L. C. Garrett.

PROCEEDINGS

OF THE

UNIVERSITY OF DURHAM

PHILOSOPHICAL SOCIETY.

EDITED BY THE SECRETARIES.

VOL. I.—1896-1900.

NEWCASTLE-UPON-TYNE:
ANDREW REID & COMPANY, LIMITED, PRINTERS AND PUBLISHERS.
1900.

CONTENTS.

PROCEEDINGS

OF THE

UNIVERSITY OF DURHAM

PHILOSOPHICAL SOCIETY.

PART I.—1896-7.

NEWCASTLE-UPON-TYNE:
ANDREW REID & COMPANY, LIMITED, PRINTERS AND PUBLISHERS.

1898.

OFFICERS OF THE SOCIETY.

SESSION 1896-97.

President:

THE VERY REV. THE WARDEN.

Vice-Presidents:

REV. H. P. GURNEY, M.A., D.C.L.

J. T. MERZ, Ph.D., D.C.L.

PROFESSOR G. H. PHILIPSON, M.A., M.D., D.C.L.

REV. A. PLUMMER, M.A., D.D.

REV. A. ROBERTSON, M.A., D.D.

REV. H. B. TRISTRAM, M.A., D.D., F.R.S.

Committee:

A. W. ASHTON.

PROFESSOR R. HOWDEN, M.A., M.B.

F. B. JEVONS, M.A., D.Litt.

PROFESSOR G. A. LEBOUR, M.A.

D. W. PATTERSON.

PROFESSOR R. A. SAMPSON, M.A.

Honorary Secretaries:

F. C. GARRETT, M.Sc. *(Coll. Sc.)*

C. SALKELD, B A. *(Coll. Med.)*

UNIVERSITY OF DURHAM
PHILOSOPHICAL SOCIETY.

INAUGURAL ADDRESS DELIVERED BY THE WARDEN

(The Very Rev. Dr. KITCHIN, Dean of Durham), as First President of the Society.

The first public meeting of a Society the name of which promises much is the right moment for asking, What is the aim it sets before itself? For philosophy is a term which has elastic edges, and may stretch over the whole realm of knowledge. The difficult paths of psychology, whether studied from the abstract or in the concrete, will clearly be included: the laws of human thought, the observation of human acts, the enquiry into natural phenomena; the application of knowledge to use, speculations as to the limits of human intelligence, the influence of human passions, the regulation of all that can amuse and lighten life; here are a thousand topics for the investigations of a philosophical society. It might almost seem as if this Society were aiming at some conspectus of the progress of human knowledge and power, like a kind of committee of the yearly meeting of the 'British Association for the Advancement of Science.' There are ages which are specially fitted for this work of review; and men who stand out in history as representatives of this task. These times and men come usually after a period of vigorous life or movement, and sum up results. We see a cyclopædic turn in them; however much people may have preached that perfection lies in limitations,

and that the man who studies a frog or a beetle, or who masters one language, or is skilful as an artist in some branch of beautiful work, is the wise man, still the human mind yearns after generalisations, and wishes to rise to some height from which to take a bird's-eye view of knowledge and correlate its several parts.

It was when the brilliant age of Greek thought was over that Aristotle undertook such a task as this. Were we to shut our eyes to history, we should find ourselves guessing that he was mapping out the field for a great advance, and saying that his treatises were the starting-point of successive triumphs in the world of thought, of action, of material knowledge. But we know that this was not so—that the master mind was too late for his countrymen, and had far less effect on them than he has since had in both the thinking world of the schoolmen and in the active world of our day. It may be that Aristotle, of so great and commanding a genius, fell short of influence through the absence of imagination and of what we may term the spiritual side of his nature. Perhaps a similar barrenness of result befell his splendid successor, after many centuries, St. Thomas Aquinas.

Another encyclopædic mind, which has rather won the fugitive glories of the orator than those of the philosopher, was Cicero, who in his smooth expanded style traversed the whole realm of learning of his time, he, too, showing therewith a sad lack of poetical spirit. One might easily draw a close parallel between him and our English thinker, Lord Verulam of St. Albans. For Cicero and Bacon were singularly alike in circumstance, though divided by a surprising chasm of differences.

Cicero came after the closing efforts of the dying Greek schools. He was, above all things, an eclectic ; the Greeks had the teaching of him, the Greeks of the decadence. As a young man he had been under the influence of the Epicureans in the person of Phædrus of Athens, he studied the developments of the new academy under Philo 'the academic,' he learnt the

principles of Stoicism under Diodotus, who lived in Cicero's house, and exercised very great influence on his plastic mind; hence he drew such philosophy as he adorned with the perfect Latin of polite life. The various schools of the philosophers had ceased to have active force; Cicero was free to make his own smooth system out of them; wherefore he was a great lawyer, a still greater orator, and no philosopher, and when he touched politics it was only to his scathe.

Bacon, too, came at the close of a period of growth and awakenment; the air was full of theological and speculative movement. He, too, was eclectic, with this difference, that, unlike the others, he saw that there was a vast future before the human mind, and that new methods were needed for them. This was the great difference for my lord of Verulam. In matter of legal knowledge, of dabbling in politics, in the enquiring mind, he was on a level with Cicero: if Cicero was besmirched with vanity and weak ambition, Bacon could bow a subservient head before the pedant of the north. If Cicero was crushed between the rolling wheels of greater men, Bacon was content to rise humbly by the favour of the first Stewart king. Here, however, the parallel ends; for, while Cicero left behind him nothing which had in it the germ of any future advance, and his works, after the Latin manner, are pale reflexes of the greater literature of the Greeks, Bacon, on the other hand, recognised and seized on those principles which had in them true life, which were destined to affect the destinies of ages to come. In other words, Bacon, though he reckoned up and appreciated the systems of the past, did not thereby become an eclectic. He was far more than that. A balancing system, which, by rubbing off all corners and deadening all startling colours, reduces half a dozen systems of life or of thought to one harmonious whole, is the triumph of the eclectic mind. It is to science what those schemes of religion aim at, when they smooth away all salient points and crush out independence, and make men think they are agreed because their beliefs are colourless. Neither in religion nor in

science is such unity worth much. Bacon sought out no such balancing scheme. Falling in with the temper of his time, he tried consciously to lay the foundations of a new palace of knowledge. Cardan had, in the latter part of the preceding century, developed mathematics by working out the system of symbols which we still use in algebra. These were to mathematics what letters were to thought generally; and the new logic of Bacon, the inductive organism which he worked out with great care, was intended to be just such another language for the fields of discovery in nature. I know no more interesting piece of autobiography than that in which in his old age he says, 'Indeed I remember me, some forty years ago, having written a youthful work on these matters, which with great self-confidence and with a high-sounding title I called "Temporis Partus Maximus."' And this was the old man's colder judgments on his own youthful ambition. But though old age might have cooled him, no less was the audacity of youth of true service to him. This 'mightiest birth of the ages' was in reality the germ of his great work, a scheme for the interpretation of nature by the new method of induction rightly treated. For it was a method which seemed likely to place all nature before the eyes of men: and set enquirers on useful instead of useless speculation and enquiry. It is true that Bacon never thoroughly worked out his plan. The 'Great Restauration,' of which the 'Novum Organum,' though incomplete, is the most solid part, was not fully carried out. It remains to a great extent a splendid suggestion; and is the germ of all the infinite patient work of those who since his day have dedicated their lives to enquiring into and interpreting the world around us. It set mankind on the road; and now, after nearly three centuries, Bacon's wise efforts are bearing infinite fruits. 'The story which ("Novum Organum," i., 85) he applies to the Alchemists, of the old man who bequeathed to his sons a treasure of gold, hid in his vineyard,' without saying *where:* so that the sons dug the whole surface up and so gave it the impetus which brought them back

a bag of gold from the quickened produce, this fable 'applies in no small measure to himself.' He set mankind a-digging in the fruitful soil, and brought them health and a rich plenty of fruit.

It seems to me that societies like this which we are launching to-night have a very wholesome use in enregistering some part of the work done and regions won. It is philosophical: neither physiological nor psychological, neither contemplative nor active, but a company of 'Lovers of Wisdom,' who will humbly and bravely build on the firm foundations already laid down by long and patient enquiry and invention, and will live in hope that we too, like Bacon, may in the upper storeys build our glass houses, that therein, with the blessing of God's sun and light, all beautiful things may grow and bear fruit.

The author of the thoughtful and very valuable contribution to the study of religion which has been published this year is here to-night with an account of science as it shows itself among savages. It is well to begin at the beginning. At any rate we shall not be so likely to lose ourselves as in the bewildering speculations of the 'Sein' and 'Nichtsein,' the 'Ich' and the 'Nicht Ich': the doubt as to existence; the difficulty of placing the *ποῦ στῶ* of human knowledge and life. We may learn that, as knowledge grows, light also grows; and that our efforts have a definite and wholesome aim, in the rise of human life, in the bettering of human character, in the solution of some at least of the problems which are still reducing society almost to despair. At least, we shall be conscious that God has made us intelligent atoms in the world, and shall be strengthened to live up to our task, and to do our duty in His sight.

SAVAGE SCIENCE.

By F. B. JEVONS, M.A., D.Litt.

[Read December 3rd, 1896.]

I hope that this hastily written essay will not be taken as giving the measure of my respect for the Durham University Philosophical Society. I would gladly have bestowed upon it the labour necessary to make it worthy of being read before the Society, but it was impossible to do so in the short space of three weeks in the midst of the work and distractions of term time. I suppose, if I had had any proper modesty, I should have declined the invitation to read an essay, when I found that I had not the time to write one properly. But in the same way all the members of the Society might have made the same excuse, and it would have been a bad beginning, if one member after another had begun to make excuse and decline to do his duty. Besides, to be quite frank, I was proud of being asked to read the first paper, and was not going to be so ungrateful for the compliment as to decline the honour.

When I began to cast about for a subject, I remembered that at the first meeting held by the promoters of this Society, the Chairman suggested, and the meeting welcomed the suggestion, that the term science should be interpreted not so as to mean the physical sciences alone, but so as to include the less exact sciences, such as anthropology and folk-lore. I thought therefore that something in the way of anthropology might be welcome to workers in other fields of science. At the same time it seemed that a scientific Society might very well in its first meeting begin at the beginning and consider what was the origin and early history of science. And, accordingly, I propose in the first place to lay before you to-night what anthropology and folk-lore have to say as to the scientific ideas of primitive man, in a word as to Savage Science. We shall then be able to consider what, if any, is the difference between savage and civilised science. And then we may conclude in a way worthy

of a philosophical Society by discussing what is the philosophic basis of science—savage or civilised.

In this place and in this Society it is not necessary—indeed it would be a piece of presumption—for me to insist that evolution is universal and that the law of continuity operates everywhere. I only wish to point out that if everything is subject to these laws, then science itself must also be subject to them: science like other things has been evolved, and the law of continuity holds good not only in science but of science. As the higher forms of life have been evolved out of the lower, man from the ascidian, by a continuous chain of indistinguishable intermediate forms, so the science of to-day has been gradually evolved out of the science of the savage: the one is continuous with the other and shades off imperceptibly into it. 'The assured triumphs of modern science are linked to the despised speculations of the savage by a chain which may be ignored but cannot be snapped.'

Every evolutionist of course will be prepared, on *a priori* grounds, to find that this is so. What anthropology and folklore do is to prove that it is actually the case—that 'the foundation, the principle, and the methods of savage logic and scientific logic are identical.' The foundation of modern science is the belief in the uniformity of nature, the belief that is, that what has once happened will happen again in similar circumstances—for instance that a cause will always produce its effect, in the absence, of course, of counteracting causes. But this belief is quite 'as strong in primitive man as in the modern *savant;* and the savage not only expects a cause to produce its effect, but also holds with Mill that a single instance of the production of a phenomenon by a given antecedent is enough to warrant the belief that it will always tend to be produced by the antecedent. Thus "the king of the Koussa Kaffirs" having broken off a piece of a stranded anchor died soon afterwards; upon which all the Kaffirs looked upon the anchor as alive and saluted it respectfully whenever they passed near it.' What has once happened, you see, may happen again—the anchor had

killed one man and might kill another. A single instance was quite enough for the Kaffirs—just as it is held by the modern man of science that one undoubted case of causation is sufficient to base a general conclusion on.

Wherein, then, lies the error in this piece of savage science? The king's death undoubtedly had a cause. So far the savage was right. And whatever caused the king's death would certainly in the same circumstances cause any other man's death. There again the savage was right. The error consisted in selecting the wrong cause: it consisted in jumping to the conclusion that the molestation of the anchor was the cause of the king's death. It seems, then, that the foundation of savage logic is quite correct and is identical with that of scientific logic; but it is in the application of this fundamental principle that the savage man of science goes wrong. The advance, then, which the modern man of science has made appears from this to consist in the fact that he has invented methods whereby to distinguish the antecedent which does produce the effect and is really the cause from other antecedents which have nothing whatever to do with the case. Now, there certainly are such methods. They are commonly known as Mill's Methods, because J. S. Mill was the first to formulate them—or as the Four Inductive Methods, because there are five of them. And the modern man of science certainly does use them 'to distinguish the antecedent, which is the cause from the other antecedents which have nothing to do with the effect under investigation.' But—*so does the savage.* There is not the slightest difference between savage science and civilised science in that respect. This, however, is a statement which it is necessary that I should prove. I will, therefore, at the risk of being tedious, take each of the inductive methods in turn and give instances of its employment by savages.

The first is that which is known as the Method of Agreement. You wish to ascertain the cause of some phenomenon, so you carefully scrutinize it whenever it occurs, and you notice that

there is one particular antecedent present every time it occurs, so you infer that that particular antecedent is the cause of the phenomenon. Thus the aurora borealis is always accompanied by considerable magnetic disturbances; and the inference is that they have something to do with causing it—all the instances of the occurrence of the aurora borealis are in agreement in that one point, viz., the presence of magnetic disturbances. But now the Peruvian mountaineers in the time of the Incas, when they descended from their healthy mountain tops into the unhealthy valleys of the coast were seized by a particular kind of sickness; and they cast about to discover the cause and they used the Method of Agreement. They observed that it was only when they were within sight of the sea that they were thus afflicted, and they scientifically inferred that the cause of the sickness was the supernatural power of the sea—that was the only antecedent uniformly present on all occasions of that particular illness, and, therefore, being the one point in which all cases agreed, it was by the Method of Agreement the cause of the illness.

But you will say everybody knows that the Method of Agreement is not a safe one—the books on logic warn you that it is not conclusive and is often misleading. If you want to be quite sure that you have hit on the right cause, you must use the Method of Difference. Very well then, let us try the Method of Difference. It is this: if into a set of known conditions you introduce a new antecedent, and immediately a new effect emerges, the new antecedent is the cause of the new effect—the only difference introduced must be the cause of the only difference that ensues. 'A bullet is fired from a gun or a dose of prussic acid is administered and an animal instantly falls dead.' The inference is that the new effect, viz., death, is due to the new circumstance introduced, viz., the gun-shot wound or the dose of poison. By means of an ingenious apparatus Professor Tyndall contrived to introduce motion into a tube of cold water until the water was converted into steam, and the inference

again was that the heat was due to the motion. In exactly the same way the method of difference 'is employed by the Dusans in Borneo who, according to Mr. Hatton, "attribute anything—whether good or bad, lucky or unlucky—that happens to them to something novel which has arrived in their country. For instance, my living in Kindram has caused the intensely hot weather we have experienced of late."'

The method of Concomitant Variations plays a still larger part in savage science. According to this method, things which vary together are causally related to one another, or *vice versa*, things which are related together vary together. Thus the size of the cerebrum, and the intelligence of an animal vary together, and the degree of intelligence, and the size of the cerebrum are accordingly supposed to be causally connected. The more friction a moving body encounters, the sooner it is brought to rest, and *vice versa* the more friction is reduced the longer the body moves—whence the inference that friction is the sole cause of the retardation of its motion. Another instance is afforded by the action of the moon upon the tides. But primitive man observed that the tides were not the only things on the earth which varied concomitantly with the moon. As the moon waxes so the crops grow: the two things vary together, and the savage accordingly infers that the moon exercises a causal influence on the growth of the plant. The seed therefore must be sown at the new moon, and then as the moon grows larger, so the plant will grow and increase. In the same way, you should count your money, when you first see the new moon, for then it will increase as the moon does; and you should never pick apples when the moon is full and about to wane, for then as the moon decreases so the apples will shrivel. And I learn from the *British Apollo*

> That when the moon 's in her increase,
> If corns be cut they'll grow apace;
> But if you always do take care
> After the full your corns to pare,
> They do insensibly decay
> And will in time wear quite away.

Again, a foot and a foot-print are obviously causally related and vary together. What affects the one will affect the other. Hence injury to one will injure the other, so the Australian aborigines bury sharp fragments of quartz, glass, bone or charcoal in the foot-prints of their enemy. In 1852 in Grafton County, New Hampshire, a knitting needle was stuck into the foot-marks of a reputed witch, in order to fasten the witch in her tracks. Elsewhere in the States an awl is prescribed. In the Alleghanies they recommend a nail from the coffin wherein a corpse has decayed to be driven with three blows into a thief's track—it will produce the same effect as if it entered the robber's foot. But you are cautioned to tie a string round the nail's head, so that it can be drawn out when requisite; else the man will die. Again, much the same relation that exists between a foot and a foot-print exists also between a man and his likeness; and by injuring the one you can inflict pain on the other—hence the witch could torture her victim by roasting or wounding a waxen image of him. It is therefore a very reasonable precaution on the part of the savage to decline to be sketched or photographed—if he consented, he would place himself in the power of the possessor of his likeness and might have reason to repent it. Thus, according to the *Times* newspaper, 'when Dr. Catat and his companions were exploring the Bara country on the west coast of Madagascar the people suddenly became hostile. On the previous day the travellers, not without difficulty, had photographed the royal family, and now found themselves accused of taking the souls of the natives with the object of selling them when they returned to France. Denial was of no avail; following the custom of the Malagasays, they were compelled to catch the souls, which were then put into a basket, and ordered by Dr. Catat to return to their respective owners.' Again the name of a man is the counterpart in speech of the man himself; and so the relation between them is established, which is necessary to bring the Method of Concomitant Variations into play. On this principle, savages

frequently keep their names a profound secret, and the safety and inviolability of the city of Rome depended on the secrecy observed as to the name of its tutelary deity. On the other hand, if you wished to commend yourself to the protection of some particular deity, you could effect your purpose by inscribing your name on the walls of his temple, or on his image, thus placing yourself perpetually under his safeguard. It is to this practice that we owe the preservation of one of the earliest of Greek inscriptions, in the temple of Abu Simbel on the Nile. And it is to a survival of this practice that we owe the disfigurement of many ancient monuments, carved with the names of tourists. That the mere utterance of a name brings you into some sort of communion with the person named is evidenced by the satisfaction with which the lover dwells upon the name of his Rosalind:

> From the east to western Ind,
> No jewel is like Rosalind.
> Her worth being mounted on the wind,
> Through all the world bears Rosalind,
> All the pictures fairest lined
> Are but black to Rosalind,
> Let no fair be kept in mind
> But the fair of Rosalind.

If the name of a man may, in the possession of an enemy, be made the means of injuring him, much more may actual parts of him, such as nail-parings, hair-clippings, teeth, etc. Hence the care which is taken all over the world to dispose of these exuviæ in such a manner that they may not fall into the hands of an enemy. But I must not weary you with instances. I will only say that if you are inclined to smile at the obvious folly and puerility with which savage science applies the Method of Concomitant Variations, you should remember that the weather is still supposed by educated people to vary with the changes of the moon; and that as to the influence of her phases on vegetation and the advisability of sowing on a waxing moon, the founder of inductive logic, Bacon himself,

thought there was something in it: videmus enim in plantationibus et insitionibus aetatum lunae observationes non esse omnino res frivolas (*De Augmentis Scientiarum*, iii. 4). So thin are the partitions between savage and scientific logic.

One of the mightiest instruments of modern science is hypothesis, and it was of equally great service in primitive speculation. A hypothesis is defined by Mill as 'any supposition which we make in order to endeavour to deduce from it conclusions in accordance with facts which are known to be real.' This is exactly and precisely what the savage does. But whereas the suppositions of the *savant* are called hypotheses, those of the savage are called myths. Thus when it is sought to account for the observed facts that the moon periodically decreases in size, and that her appearance in the sky is the signal for the departure of the sun, a savage hypothesis accounting for the facts is that sun and moon are husband and wife who have quarrelled and separated, and avoid each other; periodically, however, the moon makes overtures of reconciliation, and waxes round and portly with hope, and periodically wastes away again before our very eyes in grief at their rejection. Or the observed facts of thunderstorms are accounted for on the supposition that a jar of rain is carried by one spirit, and is smashed by the mace of another—whence the crash of the thunder and the descent of the rain.

Again, many savages—anticipating the theory of evolution—hold that they are descended from animals; and then they have to explain how it is that they themselves are human in form, whereas their ancestors were animal. They account for the change by various hypotheses. 'Thus the Turtle Clan of the Iroquois are descended from a fat turtle, which, burdened by the weight of its shell in walking, contrived by great exertions to throw it off, and thereafter gradually developed into a man.' Again, 'the Cray-fish Clan of the Choctaw Indians were originally cray-fish and lived underground, coming up occasionally through the mud to the surface. Once a party of Choctaws

smoked them out, and, treating them kindly, taught them the Choctaw language, taught them to walk on two legs, made them cut off their toe-nails and pluck the hair from their bodies, after which they adopted them into their tribe. But the rest of their kindred, the cray-fish, are still living underground.' The Cañari Indians, who live to the south of Quito, are descended from a parrot; and this is how they account for it:—There were once two brothers whose provisions were exhausted; 'the herbs and roots which they were able to collect scarcely sufficed for their sustenance, and hunger sorely pressed them, until two parrots entered their hut in their absence and prepared them a meal of cooked maize, together with a supply of the fermented liquor, which is made by steeping it in water. This happened day by day, until at length one of the birds was made captive by the brothers. When thus captured, it changed into a beautiful woman, from whom the brothers obtained the maize seed and learned the art of cultivating it, and who ultimately became the ancestors of the Cañari nation.'

Here, then, I take leave of folk-lore and turn to philosophy, to the question, What philosophic basis has science? And I do not know how better to do so, than to ask, has modern science any better basis than savage science had? Because if it has not, we must be prepared to admit that in all probability our remote descendants will be just as much amused at the notion of man's being descended from an ape-like ancestor, as we are at the idea of the Choctaws being descended from a cray-fish; and future generations may consider the hypothesis of evolution as baseless as we consider the myths of our savage ancestors.

Now, to me it seems that modern science has no other basis than savage science had—both are built on the same foundations and by means of the same instruments of thought. On the theory of evolution, we must believe that modern science has been evolved out of savage science; and the law of

continuity forbids us to believe that we can draw any hard and fast line between the two. To imagine that the modern man of science has any logical methods unknown to his savage predecessor, is, as I have already endeavoured to show, a belief that is not warranted by the facts: all the inductive methods are acted on by the savage, and there is nothing strange in that, for they only state explicitly the ways in which men actually do think and reason, and—so great is the amount of human nature in man—men think in pretty much the same way all over the world.

But all this will not shake your faith in modern science. You will still believe that the law of gravitation is true, and that the appearance of a foreigner cannot really be the cause of a heat-wave as is believed in Borneo, or give all the inhabitants cold, as is believed, in this United Kingdom, by the people of St. Kilda's. Then, what, I must ask, is the difference between primitive and modern science? There can, I apprehend, be but one answer to that question. It is that modern science builds upon experience—relies upon facts. If we refuse to believe that a stranded anchor killed the King of the Koussas, it is simply because anchors, as a matter of fact, do not cause death in that way. If we believe that having our portraits taken does us no harm, it is because we have tried it, and know by experience that the operation is not followed by fatal results. It is in this way that the erroneous notions and false hypotheses of savage science have slowly been found out to be false, and thus by a gradual process has knowledge been continually clarified, and modern science been evolved out of primitive error.

Well! I doubt whether that has been the actual process. Pre-conceived ideas are not easily eradicated—even by the evidence of the senses. Thus, I am given to understand that the experiments conducted by demonstrators and lecturers—and even professors—do not always come off as foretold by the professor. But I also understand that this does not, in the

least, shake the faith of the student: he still believes that the given causes do really produce the stated effect, but that some accident occurred, or the professor's usual skill deserted him, and hence the unexpected result. Now, the mental attitude of the savage student of science is exactly the same as that of the matriculated member of the University; if driving nails into his enemy's foot-prints does not cause him an injury, it is because he did not drive them in at the right time, or in the right way, or else his enemy was protected by some more powerful medicine, or a thousand other things—but he never draws the right inference, he never learns to correct his hypotheses by the facts of experience. Even when the real scientific explanation is put before him and carefully explained to him, he is impervious to it. Thus when Colonel Ellis was trying to explain to a West African negro the scientific theory of the production of rain, by the evaporation of moisture and so on, he was at once met by the contemptuous remark: 'Who ever saw water go up? It always comes down.' Pre-conceived ideas, I repeat, are not easily eradicated, even by the evidence of the senses, just as formed habits are not eradicated even by experience of their evil consequences.

That facts of experience do not automatically correct the errors of theory can easily be proved. We are certain that the laws of nature were the same in the time of primitive man as in our day, that the sun rose and set, and the stars moved in their courses in much the same way then as now. In a word, the actual facts were the same for primitive man as for the modern *savant;* and, if facts can automatically correct false notions, why did they not do so? Nay! we need not go back to prehistoric times for argument. At the present day, the truths of science are just as true of what happens in the Heart of Africa as of what happens in the College of Science; and, once more, if it is the facts of experience which have automatically weeded out false hypotheses in the past, why do they not teach the inhabitant of Africa the error of his ways? Perhaps you will

say that the facts are not the same for the savage and the *savant*, that the savage has no telescopes, and microscopes, no chemical or mechanical apparatus. But no elaborate apparatus is needed to show that sticking pins into a man's image does him no injury, that nails may be driven into his foot-prints and he be not a penny the worse. If the facts are not enough to teach a man that, it is difficult to see how they can have led men to cast aside savage fancies for modern science.

To say that when a man *sees* that his theories are contradicted by facts, he will give up his theory as false, may be true enough; and to say that it is because men have come to see what speculations *are* contrary to fact, that science has advanced, may also be true. But that does not explain why men come to pay this regard to facts, which is just what *must* be explained, if we are to understand how, and why savage science has been gradually cast aside in favour of modern science.

However, I am prepared to admit that man does in some unexplained way come to see that certain of his speculations must be discarded because they are contrary to fact; and that in course of time he learns to test all his speculations by an appeal to facts. Then just as Kepler, when endeavouring to determine the orbit of Mars, tried fourteen different theories one after another, and rejected them because they did not fit in with the facts, before he finally hit upon the right one; so man starts by making all sorts of guesses, of which one after another is proved by the facts to be wrong. Now this view has the advantage about it that it is an application of the theory of the survival of the fittest to the evolution of science. Just as one form of life after another has flourished on the earth as long as the conditions were favourable to it, and then when the conditions were no longer favourable, has perished and joined the ranks of extinct monsters, so in the domain of knowledge, one speculation after another has flourished as long as it was supposed to be supported by facts, and then has passed away into the limbo of myths and ruined hypotheses.

On this I have first to remark that if the conditions have changed so often, what guarantee have you that they will not change again, that your science will not go the way of all previous science? I am quite willing to admit that the speculations of the savage are wrong, but how does that prove that your speculations are right? Facts have turned up to prove that the savage was wrong, what reason have you to believe that facts will not yet turn up to prove that you are wrong? In fine, what difference is there between savage science and modern science, except this, that the former has been found out, and the latter has not yet had that misfortune?

Let me put it another way. We are all agreed that if a theory is contradicted by facts, it must be wrong. But what if it is supported by facts, if one after another every fact that turns up confirms it? Is it true? There is one such theory, and that, the one on which all science is based. I mean the Uniformity of Nature. That under the same circumstances the same thing happens, is a hypothesis which was held by the savage man of science, and which has been steadily confirmed by every event that has happened since. No fact has yet turned up to disprove *it*. May we then believe it to be true? That is to say, have we any evidence to show that the next fact which turns up will also confirm it? We have no evidence to show that the next fact will certainly confirm it. We have no evidence to make it even probable that the next fact will confirm it. We simply don't know. For the proof of this I must get the mathematicians to explain to us the theory of chances. Meanwhile and subject to correction, I will venture on an illustration. Imagine a vast ballot-box, of huge but unknown dimensions. You know of no other ballot-box like it in the remotest degree, or whether there is another, like it or unlike it. Still less do you know the principles on which either it was constructed or was filled, and you begin to draw balls out of it. The first is white. What does that tell you of the colour of the next one? Nothing, the next may perhaps be a

black ball. You draw a second, it is white; a third white one, and a fourth and a fifth. Does that constitute any presumption that the next one will be also white? Not the least. There are an infinity of balls in the box; and, for anything you know, *all* the rest may be black. You go on drawing, you draw a thousand, or a million, if you've time, and all are white. Does *that* constitute any presumption that the next one will be white? Not the least; there are countless numbers still in the box, and the balls may have been neatly packed up in millions, first a million white ones, then a million black, then a million red, you can't tell—they may have been packed that way or an infinite number of other ways. The one thing certain is that the colour of the balls you have taken out tells you nothing whatever about the balls that are still in.

There is then *no* evidence to prove the uniformity of nature: the instances—numerous though they are—in which it has 'come off' so to speak, constitute no presumption that it will 'come off' next time, just as from the white balls that have been drawn we cannot tell whether the next will be white or black. Now, some men of science say they know this: nothing *is* certain, and consequently they do not profess certainty about anything, and they only regard the uniformity of nature as a plausible hypothesis.

That is mere cant; the man who says it does not believe what he is saying. He *is* certain that what has once happened will happen again under the same circumstances. He is perfectly certain that if he puts his finger in the fire he'll get it burnt. And so are you, and so am I, and so is everybody. But there is no evidence for our belief. We take it on faith. Whether it is reasonable to take it on faith is a question which cannot be properly discussed to-night. To discuss it properly, it would be necessary to enquire whether there are any other things which it is reasonable to put faith in, though we have no evidence for them; and, if so, what they are, and what are their characteristics, and does the uniformity of nature possess

those characteristics. But without either affirming or denying that there are other things of the kind, there is one consideration we may take into account as affecting the uniformity of nature. We may consider what would be the conduct of a man who resolved not to take it on faith. He would decline to walk on the ground because the earth might not bear his weight, he would walk off a house-top because he might not fall, he would decline to eat because food might not satisfy his hunger, he would try to walk through a stone wall because he might succeed, he would go in for the Preliminary Arts Examination because he might get through. Finally, he would be locked up in a lunatic asylum—what there was left of him—and all because he wouldn't take anything on faith.

On Education and Instruction in England and Abroad.

By J. T. Merz, Ph.D., D.C.L.

[Read March 4th, 1897.]

It frequently happens that in treating a subject theoretically and professionally, whilst increasing our knowledge or our proficiency, we lose sight of the subject itself. That which prompted our enquiries and gave interest to our theories escapes unperceived, and if, after a lengthened course of study, we halt in order to survey the ground we find that we are dealing, not with the thing itself, but with some accessory phenomenon, or some mechanical out-work; we hold the shell in our hand, having lost the kernel. Examples are plentiful in science and philosophy. M. Poincaré has said of Maxwell's electric theory, which has done so much to revolutionize and extend our knowledge of electric phenomena, that it explained everything very well, only we did not learn from it what a charge of electricity was. Organic chemistry started at the end of the last century with the definite object of studying those chemical compounds which were to be found in living matter and which were the immediate bearers and products of organic life. M. Berthelot, nearly a century later, emphasized the fact that organic chemistry had become the chemistry, not of living matter, but of the carbon compounds, most of which were artificially prepared, and that it was time, by a special effort, to bring chemists back to the study of the immediate products of life in plants and animals.

The history of the study of 'mind and soul' in the sciences devoted to these furnishes many examples of a similar process. Over the analysis of logical formulæ of psychological processes,

or psychophysical measurements, the great central unity, the problem of consciousness, of individuality and personality, has been forgotten. No more glaring example of a neglect or loss of the central question could probably be found than in the great school of critical theology abroad. Starting originally with a religious interest, religion first made way for theology or religious history, these on their part for a purely logical or historical treatment, till in the end the original religious interest finds little or nothing in many of the learned treatises of that great school. A similar fate belongs to the great subject of education. It has been narrowed down to instruction—or even to cramming—the original ideal and object, that of spreading and imparting culture, being lost sight of.

I intend, to-night, to draw attention to the position which in modern times—the three great nations—the French, the German, and the English, have taken up with regard to the original larger interests of education and the narrower problem of mere instruction. In doing this I shall dwell mainly on foreign methods and notions, allowing those peculiar to this country to suggest themselves by way of contrast. I will at once draw, from the analogies just referred to, a hint how the subject may be treated. Admitted that in all special professional or theoretical treatment of great problems we are prone to lose sight of the central and main point, of the thing itself, are there no counteracting tendencies which lead us back again to the beginning, and force upon us the primitive interest or object from which we started and have strayed away? The answer is: There are two distinct influences which oppose and tend to correct the one-sided nature of all special or theoretical reasoning or professional treatment. The one is the contact with real life, the practical occupation with the object; the other is the existence and perpetuation in our language of words which always recall the existence of the thing which we have been trying to define, and which we have possibly lost in the process of definition. These two sides, practical work and the

perpetuating office of language and words* which originally denote a real thing, have operated, sometimes alone, sometimes together, in counteracting the special professional or theoretical tendency. As long as practical electricians deal and operate with electrical charges there is no danger that this quantity will be lost sight of; the medical practitioner is continually face to face with the great facts of life and death, and the problem of life will always re-assert itself as the central question in biology. The words 'mind' and 'soul' cannot be dispensed with in half, at least, of our daily literature and language; the religious wants of individuals and societies assert themselves in the face of critical theology, marking a great reality nowhere more emphatically urged than in Mr. Kidd's *Social Evolution*, and the reality, the charm of culture, the great calamity of its possible loss or decline, the painful absence of it in some well-instructed persons, will always press the question, to what extent does our education further it and help to extinguish barbarism, vulgarity and coarseness? What might have happened if the word 'ought' had or could have been actually banished from our vocabulary, as was once suggested, is impossible to say. And yet we have in French official literature on education, during three-quarters of a century, an example of the almost absolute disappearance of the word 'education.' In the place of it, we hear only of 'instruction' and 'enseignements' of schools, primary, secondary, and superior, of colleges and lycées, of academies and the great university of France.† And yet it is to French literature that we are especially indebted for marking the difference which exists between education and instruction. This subject was treated on two memorable occasions, at the time of the first Revolution and again when France had run through its

* 'La langue vulgaire cache quelquefois, sous sa simplicité apparente, des profondeurs admirables, ou se trouvent la lumière du bon sens et la sagesse de Dieu.'—Dupanloup, *de L'Education*, I., p. 5.

† 'Chose étrange, c'est l'instruction seule qui a pris depuis un demi siècle, chez un grand peuple, le nom et la place de l'education.'—*Dupanloup*, I., p. 180.

course of three Revolutions, by two men of great ability, representing quite opposite directions of thought: the first was Condorcet, the other Mgr. Dupanloup, Bishop of Orleans. The first heralds the going out of the word education, the latter its restitution in French educational literature.

Before the days of the revolutionary governments, which deserve so much credit for taking up the great question of general or public education, and which lost such a great chance when they narrowed it down to instruction, we hear a great deal more of education than of instruction.

Rousseau's celebrated novel, which set so many great minds thinking on educational matters, Pestalozzi, Kant, Goethe, Herder, etc., deals more with education, the bringing up, the development of the mind and character, 'c'est l'art de former les hommes.' The great encyclopedia of Diderot and d'Alambert has a long article on education—little on instruction. The most recent publication, the *Grande Encyclopédie*, has excellent articles on instruction, on 'Enseignement,' on schools and colleges, but only a meagre one on education! How is it that, after having had such great educational leaders as Montaigne, Pascal, Fenelon, and Rousseau, the ideal of education should have been for a time almost lost in French professional literature, till it became evident to some eminent leaders of thought that there was something wanting in modern French society, that the ideal of education was being lost sight of, and the recent movement headed by M. Lavisse has taken it up again? Larousse says in 1870, 'Malheureusement plus on avance, plus on semble vouloir identifier l'éducation avec l'instruction.'

In 1887, M. Octave Gréard, vice-rector of the Académie de Paris, published his four volumes on *Education et Instruction*. In the preface he emphasizes the former. 'Instruction,' he says, 'properly so called, has made in our time indisputable progress. The danger is that we rest content with this, and that we sacrifice the mind to knowledge. In the discussions of programmes we often forget the end of it, that which should be its

soul, "education."' The fact is that when the governments of the Revolution, in a noble spirit of patriotism and enlightenment, took in hand the great question of national education, they wished to ensure individual freedom, liberty of religious and political creeds, and they adopted, unconsciously perhaps, the dangerous formula of Condorcet, the man who preached the doctrine of the unlimited perfectibility of the human being, the author of the *Tableau historique des progrès de l'esprit humain.* Condorcet, differently judged by eminent writers, such as John Morley and H. Taine, distinguishes carefully between instruction and education. Instruction deals with knowledge positive and certain, the truths of fact and calculation—education with beliefs, political and religious. The state has a right and duty to deal with the first, but no right to direct the second. This distinction of differentiation of education and instruction is unfortunate: you inevitably drop the ancient ideal that education in every form addresses itself to the whole man, that you cannot mathematically divide the educational interest. The result has been that in French literature, till about twenty years ago, the official and professional writers deal almost exclusively with teaching and instruction; the writers on education are to be found outside of the current literature, in the *ecclesia pressa* among the followers of St. Simon, of Fourier, among the Positivists. An exaggerated and vehement protest against the centralized government system of the university came out of the bosom of the Church, in the person and writings of Mgr. Dupanloup, Bishop of Orleans, who persisted in dealing with education when others spoke only of instruction.

French thought on education has thus, in the course of this century, effected a differentiation between education and instruction, which the most recent school are again attempting to bring together. Let us see how the matter stands in Germany.

What the successive governments of the Revolution wanted to do for the French nation, viz., give it a complete system of public education, a scheme which produced an incomplete system of public instruction, the same was attempted by many a German prince in the course of the eighteenth century. The difference was, that there existed a great number of governments instead of one, that the government was in the hands of one person, mostly of an autocrat, who was, however, fortunately not unfrequently a father of his people, a well-intentioned 'Landesvater.' The capital or 'Residenzstadt' where he resided was a small town, not separated by long streets or gorgeous avenues from the country, hardly as much secluded as the castle of an English peer or country gentleman is by the park which surrounds it. Thus this 'Landesvater' lived among his people, who, in a sense, belonged to him, as did the land through them. In order to carry on the government, or rather the management, of his country, he required many servants; to get them he founded schools—for the training of jurists and officials and for the training of religious ministers, for in all Protestant countries he was also head of the Church. But, besides this, he also took notice of the fearful barbarism and brutality which had spread all over the country, in consequence of the devastating wars which had lasted for more than a century, and which had destroyed the population and their welfare. He, or perhaps not unfrequently his wiser ministers of state who had been trained in his university, listened to the teaching of a few private philanthropists, who had sprung up in various parts of Europe, notably in such countries where wars and famine had done the most havoc. None of the German states were large enough to contain all the intelligence necessary for an enlightened government, or even a successful management; besides, they were not cut off by national pride or foreign language from the rest of the world; their boundary was near at hand, and on the other side of it there were people which spoke the same language and lived very much in the same way.

It was thus that the same kind of work, that of the government or management of a people and a country, was done in many dozen, not to say scores of examples, side by side. Comparison was possible, and competition. Every business man knows how much better is the management of such manufactories were comparison and competition exist. Monopoly kills progress and prohibits excellence. No competition existed in the government of the French people, or in the management of their affairs—no monopoly existed in the management of the German-speaking people; the same thing was done, or attempted, done well or badly, in many dozen cases. The management of every state laid itself open to the easy criticism of its neighbours. Bad, unintelligent government would drive away ability; good, enlightened government would attract it. Of this there are many examples known even to Englishmen.

A further consequence of this multiplication of centres and competition of educational work was the creation of a special class of persons who travelled about from court to court, as advisers, or organizers, whose occupation was to give their opinion or help in matters of administration, state management, economics, farming, and most of all in matters of education and schools. As the type of professional adviser in the organization of the highest forms of learning, one thinks of Leibniz, the founder or planner of academies.

As the type of the popular educationalist, the Volksfreund, one thinks of Pestalozzi. But there were many dozen of men, high and low, who occupied similar positions and performed similar functions. They were not always successful in their own institutions, as little as inventors are usually successful manufacturers, but they stimulated numberless others, all through the many German-speaking countries, and gave them ideas on higher or on popular education. They extended and developed the school system, which had existed since the time of the great Reformers. These existing schools were mostly what we should call secondary or middle schools; they neither

attained the highest standard of teaching, nor did they reach down to the lowest ranks; they required developing in two directions. Leibniz, and the class of thinkers which he influenced, extended the system upwards by creating and elaborating the ideal of 'Wissenschaft,' or science and erudition combined, which became the watchword of the high school, the university, and the academy.

Pestalozzi, and the men whom he represented or influenced, stood on the other end. They wanted to make the existing school system effective in civilizing and christianizing the people, reaching the lower classes and getting hold of the masses; they knew little or nothing of 'Wissenschaft,' but they had a heart for the poor, the needy, the neglected; they were what is termed 'Volksmänner,' not 'Männer der Wissenschaft.' Both were educationalists—not specially teachers or instructors; the one looked after higher culture, the other after popular civilization. Both had to invent means how their respective ideas were to be realised, how their methods were to be spread and made practical; and so there were started training schools or seminaries, in which teachers were taught the aims of education, and trained in the best methods of teaching. These seminaries were originally what the French termed 'normal schools,' but out of it Fr. Aug. Wolf developed, in the beginning of this century, the training school for scientific and learned research—the philological seminary.

This celebrated educational, or academic institution was, in the course of this century, copied in all the different branches of science and learning. The difference between the centralizing and uniformitarian principle adopted by the one government in France, and the multiplicity of small governing centres in Germany, is obvious. In one point, however, they agreed; both systems of education were dictated or organized from above by some authority which had absolute power. Also, so far as the higher education was concerned, in both countries this was primarily dictated by the requirements of the state or the governing prince; each wanted servants, officials in great numbers, for

the government and management of the country. In France, there presented themselves, besides, great industrial problems, and great emphasis was laid by the government of the Revolution, and by the first Napoleon, on the necessity of protecting and stimulating native industries by scientific instruction. This side was quite wanting in Germany at that time. Whilst France was developing her industries, Germany was building the edifice of 'Wissenschaft,' and sharpening the tools of the higher criticism. In Germany, if we except the medical, and a small section of the legal professions, all higher intellectual education led ultimately to some position or office which was the gift of the state, the prince, or his more or less intelligent and enlightened adviser. In order to get it, the ambitious youth had to distinguish himself early by the acquisition and love of knowledge, such as the schools offered.

For a long time the system of examinations was not uniformly developed, and to a great extent the zeal and ability displayed in the school or the lecture-room, the intelligence and eagerness in taking in and digesting knowledge, counted more than the bare marks of an examination which was frequently not even competitive.

The state, or rather the minister of state, selected his servants or officers as a clever man of business, the head of a great establishment, would select them, not necessarily by advertisement, but from personal knowledge—by a kind of business tact and judgment. It was a system which was founded on the goodwill of both sides, the employer and the employed, but which is difficult or impracticable when numbers increase beyond a certain point, or where centralization and equalization have set in.

The watchword for the ambitious youth in such a state of things was knowledge, the acquisition of it, either for its own sake or as a means to preferment. The practical application of it—except in the medical, in a section of the legal, and in some subsidiary professions—was unknown to the learner, and was not in his own hands. The object of the teacher was to infuse,

the duty of the scholar was to imbibe, an enthusiasm for knowledge. The application of the knowledge acquired consisted, to a very large extent, simply in transmitting it later on to others; it was the age when good teachers were required in great numbers. This gave the tone to the universities and high schools, in which the practical or commercial usefulness of knowledge was intentionally placed in the second rank: the first and very numerous places were allotted to those who cultivated knowledge for its own sake. The state, *i.e.*, the teachers who were state officers, held out the promise of reward to those who devoted themselves with success to the cultivation and extension of knowledge, and for a long time they were able to do so. The material reward was not great, but the country was not rich, and habits and requirements were simple. The pecuniary reward was a mere competence: added to it were two other possible gains—the possibility of acquiring administrative or bureaucratic power, and that of acquiring fame through the advancement of science or learning; possibly both. The advancement of the highest branches of knowledge is a matter for genius, and it is not likely that any system will ever be found by which the production of genius, 'these sports of nature,' can be increased. But the great bulk of scientific, critical, and philological work requires only talent, and it is evident how the system just explained would tend to direct a very large amount of talent into the line of research, especially in an age and country where the first conditions for industrial application on a large scale were hardly in existence at all.

That age has passed when the Government, which became more and more centralized, could offer employment to the increasing numbers of those who passed through the high schools. The attainment and cultivation of knowledge had, however, become the traditional function of higher education, but the time came when the man possessed of knowledge had to seek a market elsewhere and to offer his knowledge to the private buyer: he had to seek private, if not public employment.

The older state of things had lasted long enough to establish the character of the higher education, though there is a growing tendency in many cases to degrade it to higher instruction (there is a minister of instruction, not of education). The high schools and universities, instead of educating men for the government and management of public work, train professionals, persons possessed of special knowledge in definite branches, but looking for application and employment not so much to themselves as to others: formerly it was the government who employed them—now it is the rich employer, or the many wealthy companies at home or abroad. A change has also set in as regards the ideal and power of knowledge acquired in the schools—the sceptical attitude towards the highest forms of human knowledge which centred in the teachings of Hume and Kant, but which, for a time, was over-ruled by the idealistic movement, has during the last fifty years completely got hold of men's minds.

Knowledge, in itself, 'Wissenschaft,' is not any more that priceless treasure on its own account that it seemed to be to the German youth eighty years ago: it is certainly not the only civilizing and educating agent—it appears, after all, only as a means to an end—it has become applied, and has a price; it is no longer priceless in the eyes of the studious multitude.

The German high schools and universities train professionals, experts, persons possessed of special knowledge in certain branches and of the means of extending such knowledge: such persons have now got a value in the market of the world: they make up the army of chemists, electricians, metallurgists, and superior industrials of all kinds poured forth annually from German high schools.

But the culture and nursing of knowledge on a large scale grew originally in consequence of the existence of an authority who required this knowledge, who could make use of it and employ it.

The German system has in a one-sided way prepared young men as experts, but it has only in rare cases taken hold of the whole man. For a short period, it was by men like Lessing, Herder, W. von Humboldt, Fichte, Wolf, and Schleiermacher, elevated to the rank of an educational agent of the highest order: it was to elevate the minds of young Germany, and it did so effectually.

The ideal of self-help, of self-government, peculiar to the training of this country, the prospect of organizing and pioneering, of colonizing, has not been before the eyes of the rising generation in Germany till perhaps quite recently. To make the most of small opportunities, to apply a modicum of knowledge to the greatest advantage, to put up with mediocrity or average ability in others, to manage large numbers possessed only of inferior talent and acquirements—in fact the task of the pioneer and colonizer, the civilizer, manager or organizer, this is distasteful to the highly trained expert. He must have an army of clerks, an army of soldiers, a laboratory of apparatus at his elbow. Where these exist he will get on and make himself useful; where they do not exist he will be at a disadvantage alongside of the more adaptive, though less thorough practical, man whose aim has been, not to learn, but to do something, not so much to acquire knowledge as to manage and to organize. The ability for the latter is not acquired without the sacrifice of some of the professional schooling: it is not the class-room but the play-ground where it is got, and it will be lost in the same degree as excessive or one-sided instruction takes the place of education, still more if an artificial system of examinations, prizes and cramming is to take the place of the liberal spirit of school and college life or the healthy discipline of the workshop. It is not likely that the English will, in their colonizing and pioneering work, come very much into contact with the French, who, after all, have hardly any surplus population to spare. With Germany, where the population has been rapidly growing, it is different. Owing to the conditions under which German

education has grown up, it is not surprising to find that she does not so much train pioneers, organizers, and teachers of men, as experts, professionals, and higher industrials of all kinds, persons accustomed to handle special problems in a masterly, scientific, and philosophical spirit, and possessed of the vast resources which the accumulated learning of the schools has furnished them. Whilst it must be admitted and strongly insisted on that knowledge gained in the way held up by the German universities, academies and high schools had, and has still, a great educational value, we are bound to point out that this instrument of education sinks to that of mere instruction wherever knowledge is acquired only as a means to a practical end. This process, degrading an educational idea to a mere instrument of instruction and training, is going on rapidly in Germany under the influence of the utilitarian spirit of the age; it is still more prominent in this country, where research laboratories are started because Englishmen hold that German experts are out-running them in the race of commerce and industry. Those who promote them must not forget that in Germany these establishments owe their existence to the traditional love for pure science and knowledge, the ideal of 'Wissenschaft'—and that without this factor such establishments will hardly produce what is expected; certainly they will lack that educational importance which they possess, or possessed abroad, and to which we must trace the finest fruits which they produced.

It is not necessary for me, and perhaps not becoming in the presence of great educationalists in this country, to say anything about English education and its ideals. By contrast, it will probably have occurred to you how differently things have been done in this country. It is the only country whose language and whose leaders in highest culture have, without exception, refused to give up the full meaning of the word education—guided, perhaps, by an instinct, that if you once begin and differentiate you lose the full sense of responsibility,

that you are in danger of retaining the shell and losing the kernel. I have, however, made the observation, that in spite of continued criticism and fault-finding in the periodical press and in the educational literature of this country whenever anyone—not excepting the late Bishop of Peterborough—delivers an address on English ways and methods, he usually finds much cause for self-congratulation.

So it may not be quite out of place for one who is himself ξένος καὶ ἄπολις to conclude by suggesting that, after all, the English people, though by no means the best *instructed* or *informed* may be, after all, the best *educated* nation in the world.

The Effect of Alternating Currents on the Frog's Heart.

By Robt. A. Bolam, M.D., and R. J. Patterson, M.Sc.

This communication is of the nature of a preliminary account of an investigation which has been undertaken as likely to throw light on the broader question of the effect of the currents on mammals—the authors being associated with Professor Oliver in a research upon the wider issue.

We have employed throughout the experiments the alternating current of the town supply, transforming as required, so that voltages from 2 to 1,000 were readily obtained.

The brain of the frog being destroyed by pithing in the usual way, the animal is fixed to a holder. The heart is exposed, freed from its pericardial bag, and the tip of the ventricle clipped in forceps, which are attached to a pivoted lever recording on a travelling smoked surface. Beneath this heart lever a signal on a shunt circuit indicates the time and duration of each shock. The electrodes of moistened sponge are applied to arm and leg at opposite sides of the body.

With currents at low voltages there is no effect on the heart's beat. In the case of a weak frog kept long in confinement the first effect is noticeable at from 25 to 30 volts, and consists of alteration in the cardiac rhythm by the substitution of short irregular beats during the passage of the current. With 50 and 100 volts further and more typical effects are produced. These are short and irregular beats during the time of stimulation and a well marked stoppage of the heart's action in the stage of relaxation—the cessation extending over a considerable period and being gradually recovered from.

With a large strong frog a current at 100 volts must be employed before any appreciable result of stimulation is arrived at, while at 200 volts a very obvious effect is always gained. A characteristic of the recommencing beat after the period of stoppage is that the contraction of the sinus venosus is always the initiator of the restored rhythm.

In our early experiments we obtained at 300 volts such a lengthy cessation of the heart's action that we deemed we had arrived at the critical voltage determining death. But after an interval—in the tracing depicted—of 17·5 seconds the beats again commenced, slowly at first, but with an ultimate return to the normal condition.

The heating and electrolytic effects of the current, not very evident at low voltages, became very marked when high potentials were reached. The tissues in the neighbourhood of the electrodes assumed a white, baked appearance, steam rose in considerable volume, and bubbles of gas were seen to be formed. The usual time of stimulation—3 seconds—was reduced to a momentary passage of current allowed by switching on and off as rapidly as possible. The naked metal electrodes which we first employed were discarded, and sponge distributors substituted—which we now use for all experiments.

The result of 1,000 volts pressure was at first sight absurdly disproportionate, the normal beat being at once resumed. But 2 minutes afterwards a stoppage of the heart, lasting 30 seconds, ensued, and was followed by a gradual restoration of the rhythm, and finally this became slowed again to the extent of a cessation which was not recovered from. Here, then, the effect though slow in appearing, was profound disturbance and ultimate abolition of the heart's action.

We have noticed in one of our experiments a curious abnormal rhythm induced by the action of the alternating current. The ventricle ceased to beat, except at lengthy intervals, all movement of the heart being auricular, except when external stimuli, *e.g.*, the application of cold normal saline

solution, provoked ventricular contraction for a single beat or for a short series of beats only. This would seem to indicate a blocking of the normal contraction wave at the junction of the auricles with the ventricles—passage of the wave occurring only at rare intervals. A similar condition has been noticed in the experiments on mammals previously referred to.

In conclusion, we believe that the effect of alternating currents is primarily on the heart and not indirectly through the nervous system. This, however, we hope to investigate further by eliminating especially the vagus action on the heart, and trust at a future period to communicate again to the Society.

[Tracings of the effects obtained were shown, and experiments demonstrating the normal rhythm of the living heart, with the effect of a current at 100 volts also projected on the screen.]

On Post-Embryonal Development, approached from a Statistical Study of the Incisor Teeth of the Horse.

By Alexander Meek, M.Sc., F.Z.S.

(Abstract.*)

An attempt was made in this paper to indicate, by a series of measurements, the changes occurring in a part during life; and though a more interesting part than the teeth might have been selected for such an investigation, the results show that changes occur in the teeth, and in the bone surrounding them, at all periods of life. The incisor teeth of the horse were chosen as the subject of investigation for two reasons: (1) I had a large series of specimens at all ages, from just before birth up to 30 years; and (2) Professor Stroud's discovery that the teeth could be radiographed in their alveoli rendered it easy to make a beginning. I have to thank Mr. Havelock, who was then working in the Physical Laboratory, for making a series of radiographs from specimens of different ages; and as the numbers seemed to indicate results of some importance, apart from the general questions the measurements were meant to illustrate, a large series was afterwards cut into sections, and the measurements repeated and extended on the actual specimens. The numbers were given in detail in the paper published in the *Veterinarian*. Here it is only necessary to present a summary of the results.

1. The *radiographs* were naturally interesting in showing that the Röntgen rays could penetrate bone and even the teeth.

* The complete paper, giving details of the measurements, appeared in the *Veterinarian* for 1897 (pp. 391 and 437). It was illustrated by radiographs selected from a series made by T. H. Havelock, B.Sc., and also by photographs by the author.

The greater density of the latter isolated them, usually very clearly, as more or less strong shadows. The cancellous network of the bone was always visible, and even the layers of the teeth could be recognised in most cases, especially in the younger specimens.

2. The *pulp-cavity* is gradually constricted, through the deposition of dentine, until it is reduced, first, to a very small tube, and then practically obliterated. The opening at the base of the tooth is at first large, but is reduced in this manner to a mere aperture or apertures, which may be terminal, or apparently, when not terminal, externo-lateral. This is the fate of the first incisors at about 15-16 years, and of the second pair at about 17-18 years. These changes tend to show that the pulp is not a permanent one.

3. The *growth of the incisors*, as shown by the numbers and curves drawn up from them, is found to be a rapid one during the years in which the horse is growing—*i.e.*, up to 5 or 6 years; and thereafter little or no increment occurs.

4. The *actual growth* of the *first lower incisor* may be said to be rather more than 35 mm. between the second and the sixth years; and this, perhaps, is not added to at all in after years.

The actual growth of the *second lower incisor* is about 55 mm. during the same period, and 2 or 3 mm. more appear to be added possibly between the the 10th and 13th years.

The actual growth of the *first upper incisor* is nearly 45 mm. between the second and the sixth years, and perhaps a millimetre or two is added some few years later.

The actual growth of the *second upper incisor* is about 50 mm. during the same period, and may possibly be added to by 2 or 3 mm. between the 10th and 14th years. The pulp therefore is not a permanent one. The after growth, if any, can only be some 2 or 3 mm. A comparison with such pictures of the teeth of the horse as appear in Gervais' *Zoologie et Paléontologie Générales* serves to show that in the reindeer epoch the teeth of the horse were much as they are to-day. A figure of an incisor,

fashioned at the root end to form a chisel-like sharp-edge, shows that it had belonged to a horse which had been pretty old. The pulp-cavity was nearly occluded.

5. The wear is not an equal one from year to year, but appears to be slightly more rapid in the earlier than the later years. It affects the third least, the second a little more, and the first incisor most of all, in both jaws. For the first lower it is a little over 2 mm. at first, and from about nine or ten years it is less than 2 mm. a year. For the second lower it is more than 1·5 in the first few years, and under 1·5 thereafter. The first upper loses about 2·25 mm. to begin with, and later about 2 mm. The second upper loses more than 2 mm. to begin with, and less than 2 mm. later. The third pair in each case, though not measured, are evidently still less worn. Baumeister gives 2 to 3 mm. as the yearly wear for the incisors.

6. Much variation occurs, as we should naturally expect, for the wear depends for one thing upon the animal. Sometimes it is readily explained by the teeth being comparatively soft or hard, or from being subjected to constant use; but the variation is often a structural one, and this *tendency to vary* is illustrated both in the numbers and their graphic representation.

7. For the accommodation of the teeth the bone is (1) absorbed in front and behind the growing teeth, and the alveolus may even be slightly added to at the margin during the emergence and meeting of the teeth; (2) each cavity or alveolus then decreases in size, due to the progressive absorption of the margin accompanying the decrease in size of the teeth, though the two processes are not absolutely dependent upon one another. This decrease in size may, however, be assisted by a late bony deposit behind the tooth (cf. *infra*).

8. The alveolar cavity is also gradually modified so as to admit of the approximation of the crowns. The teeth thus become separated at the roots in the old condition. In the younger specimens they are close together. The bony alveolar

cavity then is absorbed in response to pressure, and deposition occurs where the pressure has been removed. The more rapid reduction of the central or first incisors seems to be assisted by a late leucocyte attack from behind—cf. the radiographs of sixteen and twenty-one years,—and the alveolus seems to receive a bony deposit in consequence of this absorption of the practically dead tooth, a deposit which assists in the reduction of that cavity.

9. These changes have been shown to accompany changes in pressure, and are therefore variable, as the teeth which produce the pressure are variable from many causes. They are more pathological than developmental. The bone is formed and absorbed according to circumstances which are necessarily variable, and the deposition and absorption are variable for this reason if no other. At the same time the processes are the accompaniment of ageing. There is no static condition in this part of the body at any rate; and as the later results are beyond the pale of heredity we might be justified in ascribing not only the variations, but the average condition of late years, simply to the fate of position.

10. These facts as regards the alveoli of the incisors lead us to suppose that similar results would follow a comparative successive study of other bony elements. We are familiar in general terms with the intensification of projecting parts, and the progressive uniting of bony elements in advancing years. We know the gradual deposition and absorption which characterise growth of bone, and these processes are doubtless controlled by the conditions of pressure—the mechanical work to which they are subject. If these exceed a certain limit they lead to a pathological state; but within that limit they will cause, to some extent, a making and unmaking to suit the conditions of which they form a part.

PLATE I.

FIG. 1.—Lower incisors, 8—9 months, showing also the small alveoli of the first pair of permanent incisors and the milk canines as apparent excrescences on the side of the third milk incisors.

FIG. 2.—Lower incisors, two years, showing the progress made in the development of the first and second pairs of incisors. The lower pointed teeth are the permanent canines.

FIG. 3.—Lower incisors at three years. The first pair of temporary incisors are now replaced. The second pair of permanent incisors are coming into place, and the large alveolus of the third incisor with the contained tooth is seen.

FIG. 4 illustrates the progress made at four years. The third pair of permanent incisors are still inside their alveoli.

FIG. 5 shows the condition of the lower incisor at five years.

FIG. 6 is from the lower jaw of a pony of seven years.

PLATE II.

FIG. 7.—Lower incisors and canines (♂) at eleven years.

FIG. 8.—Lower incisors and canines (♂) at thirteen years.

FIG. 9.—Lower incisors at sixteen years.

FIG. 10.—Lower incisors at twenty-one years. The divergence of the roots is shown in these latter figures.

FIG. 11 is a photograph of a dissection of a 3-year lower jaw, and shows the large canine of the ♂, the temporary and permanent third incisors, and the temporary and permanent second incisors.

FIG. 12 is a photograph of two sections of a 1¾-year-old upper jaw; the upper is through the first milk and adult incisors, the lower is through the second milk and adult incisors. In the latter the cup is still incomplete below.

PLATE I.

1

2

3

4

5

6

PLATE II.

7

8

9

10

11

1

Native Methods of Fire-making.

By Henry Louis, M.A., Professor of Mining.

[Read March 4th, 1897.]

I feel that I am bound to commence by apologising for the somewhat delusive title of the short paper I have the honour of submitting to you to-night. It is not my intention to attempt to cover the whole range of the subject of fire-making, but rather to sketch the main outlines in the most elementary manner, and only to dwell at all upon one or two special methods that have come under my personal notice. As far as I know, there has been no exhaustive work written as yet upon the subject of savage methods of producing fire, and if anyone wishes to write such a work, he has little, if any, time to lose, because the majority of these methods are rapidly becoming extinct. It is often said that the advancing wave of civilisation may be known by the wreckage that it throws up in the shape of sardine tins and liquor bottles, and whilst it is true that it is difficult to get beyond the line thus demarcated, whenever you do, the match-box will still be found ahead, acting everywhere as the true advance guard of the white man. It has happened to me on several occasions, and in different parts of the world, to be the first European who had ever visited certain districts, and I have been in many places where a white face was rare enough to be a subject of general curiosity to the natives, and yet I have only, so far, come across one tribe that did not know the use of matches; all others, even if they did not always carry them, for economic or practical reasons, were yet quite familiar enough with them not to look upon them at all as curiosities, although they may regard their possession as a distinctive attribute of the white man, possibly a sign of his inferiority in

being unable to get fire in the ways familiar to themselves, just as the fact that we are unable to walk without boots—which are, after all, a foolishly useless encumbrance from the native point of view—excites very general wonder, not unmixed with, perhaps, a little good-humoured contempt. Significant in this connection is the fact that when a gang of Kroomen, or of Kaffirs, come down to work for Europeans, and are naturally compelled to assume names that the clerk can write on a pay-roll, one of the most usual is that of 'Matches;' you will rarely find a gang of these men that has not a 'Matches' amongst them.

Going back to the primeval methods of fire-making, all that we can assert with anything like confidence is that we have evidence that the primitive races of mankind used fire at almost as early a period as that from which the first surviving sign of their dawning civilisation dates. In this connection I need only refer to the fact that bones, some of which belonged to animals now extinct, showing the marks of fire, have been found in caves together, at times, with palæolithic implements. It is, however, in the very highest degree probable that early man used fire long before he was able to produce it, and that the utmost care was taken to preserve fire by constantly feeding it, as we still see done nowadays by some savage races that find the production of fire a piece of laborious work. As this practice of keeping a fire alive through long periods of time has thus been locally preserved among savage tribes, even to the present day, it is perhaps a legitimate hypothesis that such rites as those of the vestal fire in Roman times represented a survival of such habits from very ancient periods; it seems that more than one custom connected with fire lingered, long after its utility was at an end, as a religious or a superstitious rite.

For the purpose of convenience I propose to divide the various methods of fire-making into four groups, which seem roughly to correspond to the order of their successive development, although I am aware that my classification is far from

scientific: (i.) mechanical, (ii.) physical, (iii.) chemico-mechanical, (iv.) chemical. Of the mechanical methods, I think that only those in which heat is obtained by means of friction deserve to be thus considered; wherever percussion is employed, I hope to be able to show that the method is rather chemico-mechanical than mechanical.

A number of methods of producing fire by friction have been described, the simplest employing a to-and-fro rubbing motion, the more complicated a rotary motion. Personally, I only know one of these methods, namely, a very crude one, used by the Sakei or Jacoon tribes on the Orang Outan or Orang Bukit, as the Malays call them. All these names are to be met in literature. These are the aborigines of the Malay Peninsula, unacquainted with the use of iron, leading a nomadic life in the jungle, their clothing made from strips of bark, their weapons the deadly sumpitan, or blow-gun, and young bamboo shoots with the points hardened in the fire, their dwellings branches and leaves interlaced and woven together, their food jungle produce, *e.g.*, fruits and roots, and any animal they can hunt or snare. I once came upon a settlement of these people whilst cutting a new road across the main mountain chain of the peninsula; they were comparatively civilised by intercourse with the jungle Malays, had built huts of bamboo, had lived in the same place for over a year, and could speak a little Malay. I enquired how they got their fire and they showed me that they took two pieces of bamboo, old and hard, they split off a piece about two inches wide, cut the point half round, and rubbed it against the soft but dry pith in the concavity of the other side; of course the siliceous hard outer coating of the bamboo acted admirably to rasp the soft spongy dry fibre of the interior, which thus acts as tinder and takes fire. They did not make fire, because they considered it too much trouble; it was easier to keep going the fire they had always burning, and on jungle travels they will carry a glowing coal between two strips of bamboo, and can thus light a fire whenever they

want it. I should like to digress here to say that I have noticed that methods of obtaining fire seem to develop most rapidly among such tribes, *e.g.*, some of the American Indians, as are in the habit of smoking. I feel inclined to say that this vice, that has so often been vehemently attacked since the days of good King James, has thus possibly contributed indirectly more to the material development of the arts than have a good many virtues. To return to the Sakei encampment, it is intelligible that in a country such as the Malay Peninsula, with a rainfall of 80 inches per annum, making fire by friction is a laborious process. The skill of the savage in fire-getting is shown more by the extraordinary dexterity with which he will keep the tiniest spark alive and fan it up and work it into a blaze, than it is in producing the spark originally, and the former is far the more difficult knack to acquire, although, as I have already suggested, I believe it was acquired long before the other.

The Malay tribes, both of the mainland and of the Archipelago, are said to make fire by friction, by sawing the sharp edge of one bamboo against the convex side of another, and it has been supposed that forest fires caused by the rubbing of bamboos against each other under the action of wind, first suggested these methods. I have never seen any of them practised by Malays, because Japanese imitations of European matches are very cheap throughout the Far East and have been carried into the remotest corners by enterprising Chinese merchants, as also have flint and steel of fairly good quality. The methods of fire-getting by rotation that are best known are those of twirling a stick between the hands, revolving it by means of a thong or cord passed round it, or by means of a bow, like the jeweller's bow drill, which is still used in this country for drilling. These appliances indicate a higher state of civilisation, and, if not now in use, have survived till quite recently among savages of all parts of the world. I should just like to mention that it is said that this method is still in use

as a toy in the canton of Neufchatel, and to refer to its having been used even as late as the present century in carrying out the curious superstitious rites of the "need fire" in Scotland. Many here, no doubt, know much more than I do about this last-named subject.

Under physical methods of fire-getting must be included the use of burning mirrors and lenses, said to have been used by several partly civilised nations. The next of these methods to which I should like to direct your attention, belonging to the same group, is in some respects the most curious of all; I have classified it among the physical, but I am afraid I can give no definite idea as to where it should come in chronological order; it is known quite sporadically over a certain range in the Far East, chiefly so far among the islands inhabited by the Malayo-Polynesian race, but it is not habitually used by any one particular tribe or tribes; I am in doubt whether to attribute its wide distribution, in isolated examples, to independent invention or to a survival, but am, upon the whole, rather inclined to the latter view. This is a practical application of the scientific toy, often used as a lecture experiment, known as the fire syringe, which is used for the purpose of showing the great heat given out by the compression of air. Though it has been described by a number of travellers in Borneo, Sumatra, and other islands of the Malay Archipelago, it had never been seen in the mainland of the Malay Peninsula until I was fortunate enough to discover a specimen whilst exploring one of the very wildest parts of the Malay Peninsula, which no white man had ever reached before myself. I was camped in the jungle on the banks of a small stream, to which I had had to cut my road through dense forests, when a party of jungle Malays came down the stream, making their way back to some village with loads of jungle produce, such as damar, rattan, etc. On seeing me they, Malay fashion, squatted down for a chat, and of course used the interval either for chewing betel or for smoking. One of the party, a youngish man, lit his cigarette by means of this fire

syringe ; I happened to notice him and he showed me some half-dozen times how he did it. The syringe is a piece of hard wood, neatly plaited with strips of rattan and hollowed out into a tube ; the piston, which fitted well when in use, has a slight hollow ; into it the Malay pressed a piece of tinder which he said was made from some fungus, and of which he carried a supply in the hollowed-out skin of the seed of a large kind of bean. He forced the piston into the tube sharply, pulled it out with equal quickness and never failed to light the tinder. The other men seemed not in the least surprised at it, though they all carried the flint and steel. When I asked him for it, he immediately gave it to me, though he said he did not see why I wanted it because I did not know how to use it. He was, however, very pleased to get a flint and steel in exchange.

A few other specimens made of hard wood and more often of tin, all more or less similar, have been described, but I doubt whether there are more than a dozen or so known altogether. Mr. A. R. Wallace, whose authority on all Malayan matters is supreme, and with whom I had the pleasure of discussing the subject, told me he had come across the syringe in the Archipelago, but never on the Peninsula. I should add that this unique specimen has been described by my friend, Mr. F. W. Rudler, in *Science*, August 4th, 1893.

The next group of methods in my classification is what I have called the chemico-mechanical group, comprising the methods for obtaining fire by percussion. I call them chemico-mechanical because, as far as I know, the chemical composition of one of the two pieces that are struck together seems to be important, inasmuch as, though both must be hard, one, at any rate, must be combustible. Thus, a decided spark cannot be got by striking two pieces of flint together, but a piece of flint, quartz or quartzite, and a piece of pyrites or a piece of steel give good sparks. I have little doubt that the use of pyrites for this purpose is very old, and antedates the use of steel ; iron, as made by all primitive methods, is so soft that fragments

cannot be chipped from it, and it is not sufficiently carbonised to throw off a large and brilliant spark. Hence the art of siderurgy must have made considerable advances before it was possible to manufacture a flint and steel. Pyrites, on the other hand, was always obtainable in certain districts, and, indeed, in most parts of Europe or North America, once the savage knew what to look for, and had found out that the brown outer crust was useless, but that freshly fractured pieces would serve his purpose. Pyrites would weather and decompose to great depths so rapidly in the hot moist atmosphere of the tropics that it is not surprising to find that the geographical distribution of this method is confined to temperate and cold regions, although widely apart from each other.

The flint and steel, which was probably the most recent of the savage methods, has, as everyone knows, largely survived to the present day, in spite of the greater facility with which matches, representing the chemical stage, or highest state of development, can be used. Here, however, we have to do with far more complicated questions, which I do not propose to enter into. For example, I will only remind you that in countries in the south of Europe, where matches are a Government monopoly, the flint and steel is still largely used, for obvious reasons. Indeed there are few more convenient methods, even to-day, of getting a light for smoking in the open air, and the traveller in wild parts of the world, under trying climatic conditions, still finds it advisable to carry it. In the same way it has survived among savages and semi-savages in Asia, Africa, and America, as a matter of convenience rather than because matches are not obtainable, and will probably continue to be used for a good long time yet for these same reasons.

I have now completed the hasty survey of the subject, which is all that time admits of, and have therefore had to leave out many interesting though subordinate questions, such as those of tinder and tinder boxes. In conclusion, I beg to remind you that the very unequal stress I have laid on the various methods,

by no means indicates my appreciation of their relative importance, but has been due to a desire to contribute, as far as I could, my own observations on the subject to the general store of information concerning the art of fire-making.

The Great Ice Age.

By R. A. Sampson, M.A., Professor of Mathematics.

[Read May 6th, 1897.]

In a problem of this kind, where the data and acting causes are somewhat uncertain, but are partly liable to treatment by mathematics, there is, I believe, but one plain duty for the mathematician to perform and only one way of properly discharging it; and I venture to insist upon this because I have myself tempted fate in other fields with a mathematical theory upon such a subject. In the changes of climate which would produce a glacial epoch we are confronted by a physical problem of the utmost complexity and obscurity. The causes which modify climate, which can be used for theoretical deductions, are so little known that orthodox meteorology washes its hands of the whole of them and adheres strictly to statistics and empiric results derived therefrom. The prime agent is doubtless the sun, and regular and accountable factors are the earth's situation with respect to the sun and the earth's rotation, but the operation of these causes is modified to an extent that we can but guess at by the distribution of land and water, by ocean and aerial current, by the presence or absence of aqueous vapour or cloud in the atmosphere at any particular time. Some, indeed, hold that certain causes tend to reinforce and reproduce themselves, though evidently that is not possible beyond a certain limit. That, for instance, heavy local falls of rain, when once they have occurred, persist for a time at any rate; or what is more to our point, that accumulations of snow and ice, by means of the atmospheric conditions which they induce, assist and augment the further accumulation of the same. Now what can a mathematician do in the presence of a tangle of causes like this? As I mentioned at first, the success of all his efforts

depends upon having his causes clear. To me there appears but one place where he can make himself useful. Let him isolate any portion of the problem where the causes are clear and their operation amenable to his machinery, let him compute the consequences of these causes, and then let him lay down his tools and leave the problem, content with supplying some definite and irrefutable material for others to argue from.

Now of all the tangle of causes through which changes in the orbit of the earth might affect climate, what part, if any, is liable to calculation? This and this only, the amount of heat which falls from the sun on any particular place at any particular time. This only, out of all the questions involved, has so far been reduced to any certainty; if the mathematician proceeds to argue how much of it would be wasted by radiation and how much stored, how it will affect ocean currents or local climate, he no longer carries with him the authority of mathematics, but has embarked, in a craft built of speculations, upon waters which no one has explored.

Let us then see what calculation can teach about changes in sun-heat falling upon different quarters of the globe under different conditions that may befall the orbit of the earth. First of all, the length of the year is absolutely invariable, being subject to no permanent or semi-permanent disturbances whatever; and so is the length of the day.

Next, the areas of the arctic circles and the positions of the tropics are subject to variations so transient, or slight, that they may also be regarded as practically unchangeable.

There remain two substantial elements of change in the seasons. First, the greater or less rapidity of the sun's motion at different points of his path. And second, the situation of the day of quickest or slowest motion in the year with respect to the equinoxes, or, what is the same, to the longest and shortest days in the year. The latter of these secures that the summer, let us say, of the northern hemisphere, or the days that elapse while the sun is north of the equator, shall vary

gradually from the greatest possible to the least possible number, the greatest length of summer in the northern hemisphere corresponding to the least length in the southern, while the varying motion of the sun, due to the changing eccentricity of his orbit, affects the actual magnitude of these lengths. That is to say, summer and winter in the northern hemisphere last at present 186 and 179 days respectively; and 200,000 years ago they were respectively 196 and 169; while at a yet more remote period, 850,000 years ago, the difference between summer and winter amounted to the difference between 199 and 166 days. It must not be understood that there is a continuous change in one direction throughout this immense lapse of time. On the contrary, the eccentricity and the consequent great difference of length of the seasons has undergone many fluctuations of an irregular character, though it has never been considerably less than it is at present. Nor must it be supposed that while the eccentricity was such as to give a difference of, say, 26 days, the advantage of that length was always in favour of, say, a longer summer in the northern hemisphere; on the contrary, in the northern hemisphere at one time summer would be 26 days longer than winter; gradually this difference would diminish until after a lapse of 10,500 years winter would be 26 days longer than summer.

It is in these fluctuations that it has been supposed we may find a cause for a glacial epoch, and it is certain that if we look into it there are some very remarkable facts about the distribution of sun-heat which are well worth knowing, even if their bearing upon the glacial epoch should be less than has been supposed.

In the first place, the total heat received by the earth while the sun is north of the equator is exactly equal to that which is received while he is south, even though he should be north for 199 days and south for only 166.

In the second place, the proportion of heat which is received by the northern hemisphere while the sun is north of the equator,

compared to that which it receives while he is south, is exactly the same though he should be, as above, north for 199 days and south for 166 days. This is, in fact, the ratio 63 : 37 which Sir R. Ball insists upon with so much iteration.

Further, if we cut off the the arctic circles, exactly the same thing is true, the proportion of heat received in summer up to latitude 66° 33′ to that received in winter is invariable, and doubtless there are other curious results worth mention which have not yet been detected, but these are the chief. Now what is the bearing of this upon a glacial epoch? It appears that the ratio of sun-heat received per day in summer may bear to that received in winter, may approach as near equality as 3 : 2, and recede as far from equality as 2 : 1. At present this ratio is close upon 5 : 3. Remember that this is the heat received by the whole northern hemisphere. Moreover it takes no account of what is done with the heat when it falls upon the earth, whether it is radiated back again at once or retained; remember that in a mild open winter and in a bitter one we receive exactly the same amount of sun-heat in the above sense, and I think it will be admitted that what is done with the sun-heat when it reaches the earth is at least as important as the amount that arrives. Finally, when it appears, as Mr. Culverwell has lately shown, that if we disregard the whole hemisphere, and take a particular spot, the difference of heat received in times of greatest or least eccentricity would correspond only to a present difference of latitude say from Yorkshire to Cornwall, and I think if it is admitted that the astronomical cause may be a contributing cause to the production of an ice age, that is as much as we can assert, while far the greater part of the efficient agents must be sought elsewhere.

At this point begins a class of arguments which I absolutely decline to take seriously. I have long felt that reserve about giving or taking reasons which Falstaff so wisely exhibited. Given any two facts I do not think it is a very difficult exercise to connect them by a plausible appearance of reason, provided

only sufficiently little is known about them. Add to that, that we never meet in fiction a more desperate or unprincipled character than the scientist who sees before him some gulf in his theory which must be bridged or overleapt at all costs. One of these would divert the Gulf Stream and make the chief snowfall of the year in summer; another would be willing to slide the earth's skin about on top of its body were he not strictly forbidden by mathematicians to disturb it at all.

It may be that some of these things have happened, but I utterly distrust the arguments upon which their defenders rely. They call spirits from the vasty deep to do their work, but no one can say whether or no the spirit invoked has responded to the summons. My own view of these climatal changes, if it is of interest to hear it, is that there is a good deal of undeveloped truth in a statement which Croll embraces and which is reinforced by many others, that snow and cold tend by their presence to produce circumstances which make for their own increase; that climate is, in fact, essentially unstable or at least liable to fluctuations of long continuance and of great intensity, *without* any astronomical or other external cause to produce it. But as far as the mathematician is involved in the question, he will most contribute to respect of his kind by rigidly sticking to his last, merely restraining the geologist when he seeks to take liberties with the old earth, and leaving a geological question to be solved by geological evidence.

PROCEEDINGS

OF THE

University of Durham Philosophical Society

(ABSTRACTED FROM THE MINUTES).

Preliminary Meeting.

A meeting was held at the College of Science, on Thursday, October 22nd, 1896, to consider the desirability of establishing a 'University of Durham Philosophical Society.'

Professor Lebour proposed, Mr. E. L. Allhusen seconded, and it was unanimously resolved—'That, considering the increasing number of persons in this University interested in the progress of science, the time seems to have come when a Society might be usefully formed, having for its principal objects: (*a*) The promotion of research; (*b*) the communication of facts and ideas bearing upon scientific questions; (*c*) the exhibition of specimens, apparatus, and books; (*d*) and generally, that friendly intercourse between men working in various fields, which tends to the broadening of the views and sympathies of isolated specialists. That such Society be called the "University of Durham Philosophical Society," and that it be open to all members of the staffs of the colleges of the University, as well as to past and actual students in such colleges.'

Professor Potter proposed, Mr. Linton seconded, and it was resolved—'That a provisional committee, consisting of a Secretary and six other members, be appointed to prepare a constitution for the Society, and to arrange for the holding of the first meeting.'

The following committee was then appointed :—Mr. Ashton, Professor Howden, Dr. Jevons, Professor Lebour, Mr. D. W. Patterson, Professor Sampson, and Mr. Garrett (Secretary).

November 5th, 1896.

The first meeting was held at the College of Science on the 5th of November, Dr. Merz in the chair, when the provisional committee presented the following report :—'A meeting of the committee was held at the College of Science on Wednesday, October 28th, Dr. Jevons in the chair, when a code of rules was drawn up, and is recommended for adoption by the Society. The committee further resolved to recommend to the meeting, to be held on November 5th, that all persons present at that meeting should be original members of the Society, as also should all those persons eligible for membership who shall have informed the provisional Secretary of their desire to join the Society.'

The following set of rules was adopted :—

NAME.

I.—The Society shall be called the 'UNIVERSITY OF DURHAM PHILOSOPHICAL SOCIETY.'

OBJECTS.

II.—The objects of the Society shall be: (*a*) the promotion of research; (*b*) the communication and discussion of facts and ideas bearing upon scientific and philosophic questions; (*c*) the exhibition of specimens, apparatus, and books; and (*d*) friendly intercourse between workers in different fields.

MANAGEMENT.

III.—The business of the Society shall be managed by a committee elected at the meeting in the month of November, consisting of the officers, and six other members, of whom three at least shall be re-elected each year.

Officers.

IV.—The officers of the Society shall be a President, six Vice-Presidents, and two Secretaries (one of whom shall also act as Treasurer). The President and Vice-Presidents shall be elected annually; the Secretaries shall be appointed for two years, but one of them shall retire in each year. All elections shall be by ballot. All officers shall be eligible for re-election.

Membership.

V.—All past and present students in the colleges of the University, all past and present members of the University, and of the college staffs and councils, shall be eligible for membership of the Society. Candidates for election must be nominated in writing by two members of the Society, and their nominations must be in the Secretaries' hands at least three days before the meeting of the Society at which they are to be proposed. Elections shall be by ballot, and a candidate shall not be elected unless at least three-fourths of the votes given are in his or her favour.

Meetings.

VI.—The committee shall decide the place and time of each meeting, but in the course of the year one meeting at least shall be held in Newcastle, and one at least in Durham. The usual hour of meeting shall be 7 p.m., and the usual day the first Thursday in November, December, February, March, May, and June. It shall be the duty of the Secretaries to send each member a written notice three days before each meeting.

Subscription.

VII.—Each member shall pay an annual subscription of five shillings.

Visitors.

VIII.—Members shall have the privilege of introducing friends.

The following officers were elected for the session, 1896-7 :—

President.—THE VERY REV. THE WARDEN.

Vice-Presidents:

REV. H. P. GURNEY, M.A., D.C.L.
J. T. MERZ, PH.D., D.C.L.
PROFESSOR G. H. PHILIPSON, M.A., M.D., D.C.L.
REV. A. PLUMMER, M.A., D.D.
REV. A. ROBERTSON, M.A., D.D.
REV. H. B. TRISTRAM, M.A., D.D., F.R.S.

Committee:

A. W. ASHTON.
PROFESSOR R. HOWDEN, M.A., M.B.
F. B. JEVONS, M.A., D.LITT.
PROFESSOR G. A. LEBOUR, M.A.
D. W. PATTERSON.
PROFESSOR R. A. SAMPSON, M.A.

Honorary Secretaries:

F. C. GARRETT, M.Sc. (*Coll. Sc.*)
CHARLES SALKELD, B.A. (*Coll. Med.*)

December 3rd, 1896.

(AT THE COLLEGE OF SCIENCE, THE PRESIDENT IN THE CHAIR.)

The President delivered an inaugural address, and Dr. Jevons read a paper on 'Savage Science.'

Professor Lebour opened the discussion, and moved a vote of thanks to Dr. Jevons, which was seconded by Professor Louis.

Professor Louis said that he had visited almost all the savage tribes referred to in the paper, and suggested that the chief difference between the savage and the savant lay in their attitude towards experiments; he quoted from his own experience with Chinese miners and other savages to prove that they were not so wedded to their superstitions as Dr. Jevons had suggested.

Professor Brady suggested that drawing conclusions from incomplete data was a fault to which the physician was less prone than his patient.

Professor Sampson argued that science could be made exact by the elimination of hypotheses, but that it would be disconnected and uninteresting.

Professor Wright, Dr. Percival, Mr. Burkhardt, and Mr. Chaplin having also spoken, Dr. Jevons, in reply, contended that savages experiment boldly, for did they not discover what was good to eat, and what animals could be domesticated?

Professor Potter then exhibited some experiments to illustrate the transpiration of plants, and also a very beautiful series of drawings of the diseases of plants.

February 4th, 1897.

(AT UNIVERSITY COLLEGE, DURHAM, THE PRESIDENT IN THE CHAIR.)

The following candidates were elected members of the Society:—

REV. H. E. ELLERSHAW, M.A.	ALAN EDGAR MUNBY, B.A.
JOHN LOCKE LOVIBOND, B.A.	STANLEY WATSON.

DAVID WOOLACOTT, B.Sc.

Dr. Merz read a paper on 'Education and Instruction in England and Abroad,' and a discussion followed, in which Professor Louis, Mr. Garrett, Professor Wright, Mr. Burkhardt, and Professor Sampson joined.

The President exhibited a German war medal struck by Frederick the Great; a 'teece,' or leaden seal, of about 1500 A.D., which was formerly affixed to all sacks of wool brought into Winchester; and a document of the year 1303 A.D., bearing the great seal of King Edward I., permitting certain lands in Lincolnshire to be held in mortmain by the Abbey of St. Karileph at St. Calais in France.

March 4th, 1897.

(At the College of Science, Professor Philipson (Vice-President) in the Chair.)

The following candidates were elected members of the Society :—

Miss A. F. Bryant.	W. K. Hilton, M.A.
Miss E. B. Clark.	William Philipson, Jun., A.Sc.

A special 'Geological Photographs Committee' was appointed, consisting of Mr. Garrett, Professor Lebour, Professor Louis, Mr. Shaw, and Mr. Stobbs, with power to add to their number.

A paper was read by Dr. Bolam and Mr. R. J. Patterson 'On the Effect of Alternating Currents on the Frog's Heart,' and was illustrated by experiments.

Mr. Meek read a paper 'On the Post-Embryonal Development of Teeth in the Horse,' illustrated by specimens and radiographs prepared by the author and Mr. Havelock.

Professor Louis also read a paper on 'Primitive Methods of obtaining Fire.' In the discussion which followed Professor Stroud demonstrated the method of using the fire syringe, and Professor Potter exhibited an old 'steel mill.'

Principal Gurney exhibited a case of models of diamonds lent by Mr. Gregory.

Thursday, May 6th, 1897.

(At the College of Science, Dr. Merz (Vice-President) in the Chair.)

Professor Thomas Oliver, M.D., was elected a member of the Society.

Professor Sampson read a paper on 'The Ice Age.'

Professor Labour thanked the reader for his paper, and expressed a wish that it might be published; he also referred to the great value of the work of Croll on this subject.

Dr. Merz wished to know what was Sir Robert Ball's explanation of the causes of the ice age.

Professor Sampson replied that he did not appear to have offered any explanation. Professor Sampson also endorsed Professor Lebour's reference to Croll's work, and, in answer to Mr. Garrett, said that the fall of temperature required to produce a glacial epoch had been estimated at from 18 degrees to 45 degrees—a sufficiently wide range.

Professor Lebour exhibited a number of rock cores from a diamond-boring put down recently at East Rainton. These cores were remarkable as exhibiting sections of the Lower Coal-measures—a division of the Carboniferous series seldom worked in this district—and especially as comprising a thin bed of shale 148 fathoms below the Hutton seam, in which a *Productus*, a *Discina*, and some worm-cases were visible. This is the first recorded occurrence in Durham of such marine fossils in these beds.

LIST OF MEMBERS OF THE SOCIETY.

* *Denotes an original member.*

*Allhusen, E. L., B.Sc.
*Armour, A. L.
*Armstrong, H. E., M.D.
*Ashton, A. W., B.Sc
*Atherton, W. H., B.Sc.
*Baker, T.
*Beanlands, A., M.A.
*Beckingham, E. L., B.Sc.
*Bedson, Professor P. P., M.A., D.Sc.
*Bolam, R A., M.D.
*Brady, Professor G. S., M.D., LL.D., F.R.S.
*Brown, G. M.
Bryant, Miss A. F.
Bryant, Miss E. M., B.Sc.
*Bullerwell, J. W., B.Sc.
*Burkhardt, G. H., B.Sc.
*Campbell, William, A.Sc.
*Caunt, G. W., B.A.
*Chaplin, G. P., B.Sc.
Clark, Miss E. B.
*Cooper, J., B.Sc.
*Cross, H. E., A.Sc.
*Dale, Sir David, Bart., D.C.L., D.L.
*Duff, Professor J. W., M.A.
Dunn, J. T., D.Sc.
Ellershaw, Rev. H., M.A.
Elsdon, E. J., A.Sc.
*Fowler, Rev. J. T., M.A., D.C.L.
*Garrett, F. C., M.Sc. (*Secretary*).
*Gravell, John.
*Gray, W. R. H., M.A.
*Gurney, Rev. Principal H. P., M.A., D.C.L. (*Vice-President*).
*Hacking, Thomas.
*Hallaway, R. R., B.Sc.
Hardie, T.
*Havelock, T. H., B.Sc.
*Heawood, P. J., M.A.
Hilton, W. K., M.A.
*Houldey, W. B.
*Howden, Professor R., M.A., M.B.
*Jessop, Professor C. M., M.A.
*Jevons, F. B., M.A., D.Litt.
*Kitchin, The Very Rev. Dean, M.A., D.D. (*President*).
*Knight, R.
*Laws, A. R.
*Lebour, Professor G. A., M.A.
*Linton, A., B.Sc.
*Louis, Professor Henry, M.A., A.R.S.M.
Lovibond, J. L., B.A.,
*McConnell, W., Jun., A.Sc.
Male, F. J., B.Sc.
*Marston, Rev. H. J. B., M.A.
*Meek, A., M.Sc.
*Mellanby, A. L., B.Sc.
Merivale, J. H., M.A.
*Merz, J. T., Ph.D., D.C.L. (*Vice-President*).
*Morris, Alan C.
Munby, A. E., B.A.
Mundella, V. A., B.Sc.
*Murray, Professor George, M.A., M.D.
*Myers, J. E., Ph.D., M.Sc.
Oliver, Professor Thomas, M.A., M.D.
*Patterson, D. W., M.B.

*Patterson, R. J., M.Sc.
*Pearce, Rev. R. J., M.A., D.D.
*Percival, A. L., M.D.
*Philipson, Professor G. H., M.A., M.D., D.C.L. (*Vice-President*).
*Philipson, John.
Philipson, W., A.Sc.
*Plummer, Rev. A., M.A., D.D (*Vice-President*).
*Potter, Professor M. C., M.A.
Proctor, A. H.
*Redmayne, R. R., M.A.
*Richards, W., A.Sc.
*Robertson, Rev. A., M.A., D.D (*Vice-President*).
Rollin, C., B.Sc.
*Ross, W. D., A.Sc.
Salkeld, C., B.A., M.B. (*Secretary*).
*Sampson, Professor R. A., M.A.
*Shaw, Saville, M.Sc.
*Shelley, Miss R. B., B.Sc.
*Shortt, Rev. J. R., M.A.
*Smith, R. Greig, M.Sc.
*Somerville, Professor William, M.A., D.Œc., D.Sc.
*Stobbs, J. T., A.Sc.
*Stockdale, H. F.
*Stroud, Professor Henry, M.A., D.Sc.
*Terry, C. S., M.A.
*Todd, J. J., A.Sc.
*Tristram, Rev. Canon, D.D., F.R.S. (*Vice-President*).
*Turnbull, W., M.B.
*Urwick, W. E., M.A.
*Wade, Thomas
Walker, Rev. Dawson, M.A.
Watson, Stanley
*Watson, F. B., M.A., B.Sc.
*Weighton, Professor R. L., M.A.
Woolacott, David, M.Sc.
*Wraith, H. O.
*Wright, Professor Mark R., M.A.
*Young, W. H.

UNIVERSITY OF DURHAM PHILOSOPHICAL SOCIETY.

Income and Expenditure Account. Session 1896-97.

INCOME.	£	s.	d.	EXPENDITURE.	£	s.	d.
To Subscriptions— 95 at 5s.	23	15	0	By Printing and Stationery	3	6	0
				,, Postages	1	2	7
				,, Advertisements ...	2	1	0
				,, Clerical Assistance ...	0	9	0
				,, Expenses of holding Meetings	3	0	6
				,, Sundries	0	3	6
				,, Balance in Treasurer's hands	13	12	5
	£23	15	0		£23	15	0

Examined and found correct.

HENRY LOUIS,
H. F. STOCKDALE, } *Auditors.*

November 24, 1897.

PROCEEDINGS

OF THE

UNIVERSITY OF DURHAM

PHILOSOPHICAL SOCIETY.

VOL. I., PART 2.—1897-8.

NEWCASTLE-UPON-TYNE:
ANDREW REID & COMPANY, LIMITED, PRINTERS AND PUBLISHERS.

1898.

OFFICERS OF THE SOCIETY.

SESSION 1897-98.

President:

THE VERY REV. THE WARDEN.

Vice-Presidents:

REV. H. P. GURNEY, M.A., D.C.L.
F. B. JEVONS, M.A., D.Litt.
J. T. MERZ, Ph.D., D.C.L.
PROFESSOR G. H. PHILIPSON, M.A., M.D., D.C.L.
REV. A. PLUMMER, M.A., D.D.
REV. H. B. TRISTRAM, M.A., D.D., F.R.S.

Committee:

A. W. ASHTON, B.Sc.
ROBERT A. BOLAM, M.D.
W. CAMPBELL, A.Sc.
PROFESSOR R. HOWDEN, M.A., M.B.
PROFESSOR G. A. LEBOUR, M.A.
PROFESSOR R. A. SAMPSON, M.A.

Honorary Secretaries:

F. C. GARRETT, M.Sc. *(Coll. Sc.)*
G. G. TURNER *(Coll. Med.)*

University of Durham Philosophical Society.

On Graptolites.

By **Alexander Meek, M.Sc., F.Z.S.**

[Read December 2nd, 1897.]

The excellent material recently reported on from Sweden and America gives a much clearer idea than was before attainable of the morphological relationship of these very curious fossils. The structure and mode of growth of some of the more primitive forms have been studied by Dr. Gerrard Holm,* from specimens he was enabled to isolate from limestone; and the more differentiated Diplograptus is well illustrated in the plates published in a paper by Dr. R. Ruedemann.†

From a consideration of these papers and the work which has been done in this country and on the Continent with much less satisfactory material, we find that a restatement of the relationship of the forms and a consequent rearrangement from the simpler to the more complex agrees very closely with their succession in time.

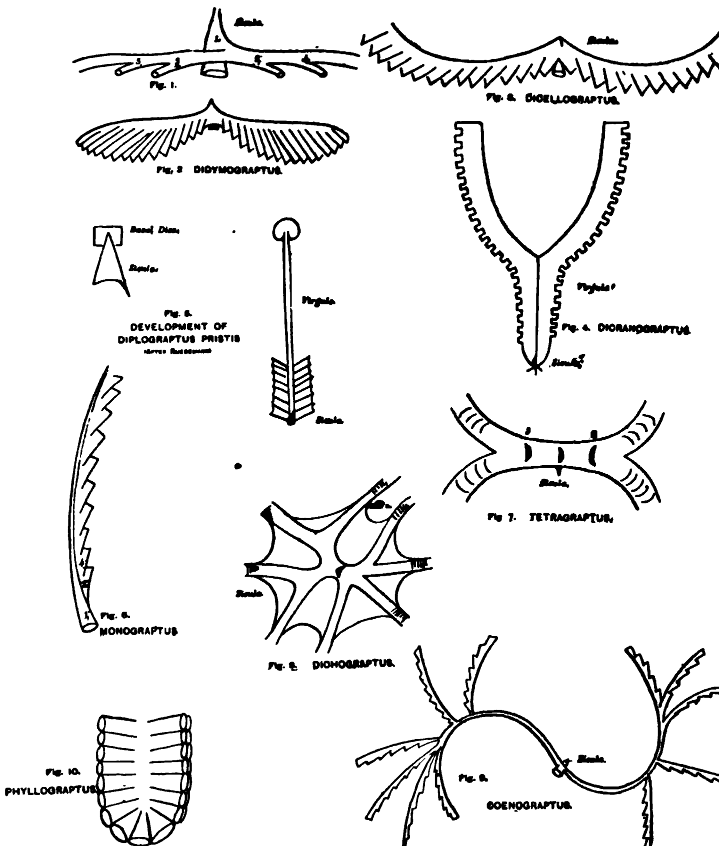
The starting point in growth in every case is a conical chitinous structure, which has been called the sicula. In such a form as Didymograptus this budded off a theca on one side, and from this second theca a third was developed in such a way as to tend to preserve the bi-lateral symmetry (Fig. 1). A

* 1895. Translation in the *Geological Magazine*, new series, vol. II., pages 433 and 481.

† 1894. 'Development and Mode of Growth of Diplograptus, McCoy.' *State Museum Report of the University of the State of New York* (48th Report, vol. II., page 219).

continuation of successive and similar buddings led to the formation of the complete colony (Fig. 2). The sicula was thus crossed on one side by the proximal end of the third theca. The crossing part has been called the connecting canal. The two arms or stipes were thrown back in such a way as to bring the secondary thecæ into a divergent position with regard to the first theca or sicula in Dicellograptus (Fig. 3). A continuation of this tendency to approximate the two branches or stipes dorsally, led through Dicranograptus (Fig. 4) to the culmination—the meeting and fusion in Climacograptus and Diplograptus. But in these forms we now meet with a curious and characteristic modification of the sicula. The smaller end of the latter is continued between the fused arms as a canal—the virgula (see Plate III. of Ruedemann's communication). Ruedemann has shown that the sicula in the case of *Diplograptus pristis*, Hall, presents a basal disc or 'basal cyst,' that is to say, a disc-like expansion at the small end of the cone. From near this disc the wall of the sicula was lengthened at the same time that lateral thecæ were developed near the apertural part of the sicula (Fig. 5). The virgula, then, is simply a continuation of the wall of the sicula. The basal disc was, as will be seen presently, for the purpose of attachment. It occurred at what must now be called the proximal end of the virgula. It has often been seen in Diplograptus and is quite visible in specimens gathered by the writer at Dobb's Lynn. That the disc was for attachment is clearly shown in Ruedemann's account of the remarkable method of multiplication in Diplograptus. From the virgula and near to the basal disc four sac-like outgrowths occur. In these 'gonangia' siculæ were developed and ultimately liberated. Some of these siculæ attached themselves to the parent stipe, usually to the basal disc, thus forming a colony of stipes. This was repeated, and led to the formation of a colony of Diplograpti of different generations. Ruedemann's Plate (Fig. 5) demonstrates very clearly that these colonies must have been fixed. It shows

part of a slab with many colonies lying so naturally together that they must have been preserved in place. Others of the siculæ escaped from the colony and started *de novo.* Free siculæ are very common in the shale from Dobb's Lynn.



The author has not seen any evidence which points conclusively to a fixed condition for Monograptus and its ally, Rastrites; but the continuation of the virgula beyond the thecæ makes it probable this was the case. Monograptus seems to have been

a late developed form from the suppression of the third theca, and the thecæ developed from it. This had the advantage of allowing a better development of the virgula.

Returning to the primitive Didymograptus, we now find a series of branching forms which have a very simple relationship with that type. Tetragraptus was developed, for example, from a splitting up and separation of thecæ four and five (Figs. 1 and 7). Dichograptus presents eight branches developed in similar manner (Fig. 8). Coenograptus is still further branched (Fig. 9). Phyllograptus results from the arms of the Tetragraptus-like type being all brought together dorsally, as is the case in Diplograptus with two branches. In Phyllograptus, however, the virgula was not developed, the sicula was buried—only the aperture appearing at one end of the compound organism.* (Fig. 10.)

The foregoing account of the morphology shows :—

(1) That in all cases the Graptolites originated from a single theca, the sicula.

(2) That the sicula came to occupy usually a reversed position in relation to the thecæ, which were developed from it.

(3) That the virgula was developed in connexion with the budding being directed towards the proximal end of the sicula.

(4) That the virgula expanded at the proximal end into a disc for attachment, and the subsequent growth of lateral thecæ led to the continued growth of the virgula, so as to keep the aperture of the sicula terminal. The virgula then was continuous with the sicula, and the area of growth was near to the point where it expands into the basal disc. It is remarkable that the chitinous wall of the virgula possessed this power of continuous growth, or of stretching and being added to by intussusception.

(5) That the virgula was not developed in such forms as presented two arms, nor in Tetragraptus, Dichograptus, Coenograptus, and Phyllograptus, and all graptolites following their

* Holm, *loc. cit.*

line of departure. The small, ill-developed pointed end of the sicula in most of these forms does not lend much support to the supposition that they were fixed. The proximal end of the sicula was altogether buried up in Phyllograptus, and no means of attachment are visible. A central disc has been figured for Dichograptus, and if developed from the sicula, as is from its position more than likely, we might assume with much justification that it was fixed like Diplograptus. With this and similar exceptions, all the forms mentioned above do not seem to have possessed a means of fixing themselves, nor any structure which could be assumed to sustain them in the water. As far as our present knowledge goes, then it must be supposed that they had the power of movement and temporary attachment with whatever the living contents of the thecæ provided. The writer imagines, in short, that the more primitive were not very securely fixed by means of the pointed end of the sicula, and that they had the power of crawling as well—say, by means of tentacles or pseudopodia; and from these types two lines of departure occurred (1) towards purely crawling forms—as Phyllograptus—which do not present any means of attachment, and (2) towards more purely fixed examples, particularly in those presenting a virgula.

(6) That they were reproduced in a remarkable manner—at least this is so in *Diplograptus pristis*—from siculæ developed in gonangial-like capsules. These siculæ are preserved in the capsules as chitinous remains. The starting-point in growth in all was a sicula, and it may be inferred therefore that the siculæ were developed in all in much the same manner as they were in Diplograptus. Nicholson has given the name *Dawsonia* to chitinous pouches of various shape found in graptolite shale, and he assumes them to be freed ova capsules. Hall has figured *Diplograptus Whitefieldi* with gonangial-like expansions.

These may have contained ova, but until more of these are investigated it will be impossible to assign such remarkable forms to a definite place in the zoological series. The extra-

ordinary asexual method of multiplication by means of the capsules of siculæ—for asexual we must, as yet, presume the siculæ developed in *Diplograptus pristis* to be—together with the remarkable origin of the lateral thecæ in such more differentiated forms have no parallel so far as the writer knows in known living or other fossil organisms. It is to be expected, however, that an investigation of the reproduction of the more primitive, *e.g.*, Didymograptus, will result in showing that we have here an attempt at colony building of a lowly type allied to the *Hydrozoa*. The colony building was evidently handicapped by the necessity for the essentially fixed ones to keep up a weak portion near the base—for lengthening the stalk; the virgula in fact was ill-adapted for supporting a colony.

(7) It would be easier to recast the classification in the light of the preceding statements. It is clear at any rate, that if the reading of the relationship given above is correct, the old division into Monoprionidæ and Diprionidæ must be given up, and that the branched forms must be removed from Monograptus and its allies. A primary division into (1) those possessing a virgula, and (2) those without one might be suggested: but the task of re-classification I leave to others better acquainted with the genera, species and varieties.

The writer begs to take this opportunity of expressing his indebtedness to Miss K. A. Smith for converting his rough sketch into the figure which accompanies this paper.

The Combination of Copper and Sulphur.

By J. Cooper, B.Sc.

[Abstract of a paper read December 3rd, 1897.]

When finely divided copper (prepared by reducing powdered copper oxide by means of coal-gas) is mixed with flowers of sulphur, and a small quantity of carbon bisulphide added, a vigorous reaction takes place, and in some cases sufficient heat is evolved to make the carbon bisulphide boil. The action is most vigorous when four parts of copper are mixed with one part of sulphur. Reduced copper was then treated with a solution of sulphur in carbon bisulphide containing an excess of sulphur, when chemical union took place and a product obtained which contains 79·16 per cent. of copper—cuprous sulphide containing 79·68 per cent.

Similar experiments were made with other metals in place of copper, either by mixing the finely-divided metal with flowers of sulphur and then adding carbon bisulphide, or by immersing a strip of foil in a carbon bisulphide solution of sulphur. It was found that zinc, mercury, iron, and silver give sulphides under these conditions, but that aluminium and platinum do not.

It has been suggested that a solution of sulphur in carbon bisulphide might be used as a lacquer for copper, but the deposit is removed so easily that it is not suitable for this purpose.

On Ductless Glands and their Internal Secretions.

By George R. Murray, M.A., M.D., F.R.C.P., Heath Professor of Comparative Pathology.

[Abstract of a paper read February 3rd, 1898.]

I propose to speak this evening for a short time on a subject which of late has attracted the attention of physiologists and pathologists to a considerable extent: the ductless glands and their internal secretions. I shall be obliged to use as illustrations some facts which belong rather to medicine than to pathology, but which are so germane to my subject that they cannot be omitted. One of the great objects of this Society is to enable those members of the University who are engaged in research work to bring forward the results of that work before their fellow-workers in other branches of this University, so that the results detailed from time to time at these meetings give a good indication of the kind of work that is being done. I wish in passing to emphasise the fact that research is undoubtedly an important part of the duty of every University, and the more it is encouraged the better for the University. I propose, therefore, after a few introductory remarks, to devote the greater part of the time at my disposal to bringing before you the results of experimental work carried out by myself in Newcastle in connection with one of the most important of the ductless glands, the thyroid gland, and its internal secretions.

The secretory glands of the body may be divided into three classes:—1. Those which have an external secretion; 2. Those which have an internal secretion; 3. Those which have both an external and an internal secretion.

As examples of the first class may be mentioned the sweat glands, with the secretion of which we are all familiar on a hot

summer's day, or the salivary glands, whose secretion may be copious after fasting at the sight of food, when the mouth is said to "water." In both sets of glands the epithelial cells secrete a fluid which is carried away from the gland by a definite tube or duct.

In the second class we find the thyroid gland, and probably also the supra-renal bodies and others. These glands have no duct, the secretion being absorbed and carried away to other parts of the body by the lymphatics or veins.

In the third class we may place the liver, which secretes bile, its external secretion, into the intestine, while the glycogen, transformed into sugar, passes as an internal secretion into the blood stream. The pancreas also belongs to the same class, its external secretion being also discharged by its duct into the intestine, where it plays an important part in the digestion of food. There is strong evidence to show that there is also an internal secretion, passing into the blood, which is in some way connected with the using up of the sugar, for we know that the removal of the pancreas from an animal is followed by the development of acute diabetes; and that this can be prevented by grafting the pancreas under the skin, so that the external secretion escapes from the body altogether, and only an internal secretion could be absorbed.

We will now pass to the consideration of the thyroid gland, which consists of two lateral lobes joined by the isthmus lying across the front of the neck. In microscopical examination we find the structure to consist of closed alveoli lined by cubical epithelial cells. These secrete the colloid substance which fills the central portion of the alveolus. This secretion escapes into the lymphatics either by small passages lying between the epithelial cells or by the rupture of the alveolar wall. This structure is shown in the lantern slides. Now, we can learn a great deal about the uses of the secretion by seeing what happens when the gland is removed and the supply of secretion cut off. In fishes no effect has been observed. In lizards there

is sleepiness, slowness of movement, and death. In snakes progressive loss of power, casting of the slough, and death. In rabbits there may be acute symptoms, tremor, muscular rigidity, and dyspnœa, ending fatally in a week, or, as in some of my own experiments, a slow development of symptoms during a year or more. In these there was gradual development of hebetude, increase in bulk, dry skin, and loss of hair, as shown in the slides before you.

So also it has been shown by Horsley that similar symptoms gradually appear in the sheep and donkey. The most important results have been obtained with monkeys; and it has been found by Horsley and by myself that in these animals the symptoms following the removal of the gland closely resemble those which occur when it is diseased in man. In monkeys, after the fifth day acute nervous symptoms appear unless the animal is kept at an exceptionally high temperature, when they are deferred. There are tremor, clonic spasms, loss of power, epileptic fits, raised and irregular temperature, followed by a fall below normal, mental dulness, decrease in number of red corpuscles, increase in the number of white, myxoedematous swelling of the skin, especially of the face, as shown in the accompanying slides. Death follows in three or four weeks. It was by these experiments that Horsley was able to show that myxoedema in man was due to disease and loss of function of the thyroid gland.

This condition of myxoedema occurs in man when the thyroid gland is rendered useless by disease. These slides show you such a gland, which has undergone fibrosis, and in which scarcely any of the original gland substance remains. The same condition results when the gland has been entirely removed by operation for goitre. In man the most important symptoms are myxoedematous swelling of the subcutaneous tissue, loss of hair, subnormal temperature, loss of mental and bodily activity, and in some cases insanity. The appearance of such cases is well shown in the slides. These symptoms in animals and man

having been proved to be due to loss of function of the thyroid, attempts were next made to restore that function. Thus Von Eiselsberg showed that in animals the usual symptoms induced by the removal of the thyroid could be relieved by grafting the gland into another part of the body; and Horsley suggested the treatment of myxoedema in man by grafting a piece of healthy gland tissue in order to restore the lost function. This suggestion was acted upon by two Spaniards, who noticed an immediate improvement in a case of myxoedema thus treated, which they attributed to the absorption of the juice of the grafted gland—an improvement, however, which in some cases has only been temporary, owing to ultimate atrophy of the grafted gland. This led me to suggest and carry into effect the treatment of myxoedema by the continued internal administration of an extract of thyroid gland, in order to maintain a constant supply of the lost internal secretion.

The chart I now show you illustrates the effect of injections of thyroid extract in a monkey, in which myxoedema had been induced by removal of the thyroid gland, and you will see that under this treatment the symptoms all disappeared. The same holds good in man; and you will see from these slides of cases of myxoedema, before and after treatment by thyroid extract, how completely the lost function of the thyroid gland can be restored by the continued administration of thyroid extract, and you will now readily understand that unless the supply is continued permanently the symptoms will return.

Cretinism is simply myxoedema occurring in childhood; and these slides will show you the good results of the thyroid extract in this disease.

In conclusion, I wish to draw your attention to the compensating hypertrophy which a portion of the thyroid gland undergoes when the rest of it has been removed. From this slide, taken from a specimen of my own, you will see that under these circumstances the cells lining the alveoli become columnar instead of cubical, there is folding of the alveolar wall, and the

number of alveoli is increased to enable the small remaining portion of the gland to carry on the work of the whole. It is of great interest that similar changes are found in the thyroid gland in another disease, exophthalmic goitre, in which there is strong evidence to show that the symptoms are due to an over-activity of the secretory function of the gland.

I trust that the facts I have brought before you are sufficient to show that the ductless glands play a most important part, by means of their internal secretions, in both health and disease.

NOTES ON THE GEOLOGY OF FINLAND.

BY PRINCIPAL H. P. GURNEY, M.A., D.C.L., F.G.S.

The Seventh International Geological Congress was held in St. Petersburg in September, 1897. It was at least as successful as any of its predecessors. The Organizing Committee spared no trouble to make the best possible arrangements. Every mineralogist and geologist in the Russian Empire, from the President of Honour, His Imperial Highness the Grand Duke Constantin Constantinovitch, the President of the Imperial Academy of Science, down to the students in the Universities, welcomed the visitors with heartiness and enthusiasm, and vied with each other in their efforts to show them all that was best worth seeing in their special districts. Expeditions were conducted to the Caucasus, the Urals, Esthonia and Finland. The last-named was one of the most popular. More than 150 representatives of nearly a score of nationalities entered their names for the excursion to the Grand Duchy. The great interest attaching to the remarkable development of pre-Cambrian crystalline rocks in the beautiful Land of Lakes was enhanced by the personal interpretation of the able and courteous Director of the Geological Commission of Finland, Dr. J. Sédérholm. Prof. Baron De Geer and Drs. Frosterus and Hackmann expounded the glaciation of the mainland, and Drs. Ramsay and Berghell took charge of parties during the visit to the island of Högland.

A remarkable appearance of persistence of land towards the north has long been recognized. All Finnish and Swedish geologists are agreed that the formations observed in the whole territory of Finland and Eastern Sweden are of pre-Cambrian age. There is no trace of mountain-making processes, nor of any considerable eruption of igneous rocks in this region during post-Cambrian and the youngest pre-Cambrian times.

While in other districts geographical changes have over and over again completely transformed the surface of the globe, these northern lands appear to have more or less successfully defied the inroads of the sea. If they have been submerged, all traces have been removed. During long ages, the rocks of this most ancient continent have been continuously worn away. They have furnished materials out of which many thousands of feet of newer strata have been in turn constructed. For the most part they are sedimentary derivatives from yet older beds, and originally they must have been of enormous thickness, probably greater than that of all post-Cambrian formations.

More than once, with patriotic pride, Finns have boasted of the extreme antiquity of their fatherland. In a speech at a banquet at Helsingfors in honour of the visit of the members of the International Geological Congress, Mr. Mechelin, President of the Municipal Council, went so far as to trace a certain harmony between the geological structure of Finland and the national character. In proposing the toast of the members of the Congress he remarked:—"Il y a une certain harmonie entre la formation géologique de la Finlande et le caractère national. Établis sur une terre solide qui n'est exposée ni à des tremblements ni à des éruptions volcaniques, nous sommes une race tranquille qui, s'appuyant sur ses bonnes lois, n'abandonnera jamais le chemin de l'évolution normale."

It is then to this far-off period, the most remote in the record of the rocks, that the Geological Commission of the Grand Duchy refer the chief formations of Finland. Followed into Lapland, beds of Cambrian age are found to rest upon them unconformably. They are classified in two great groups. The younger of these, which includes pre-Cambrian sediments, which are still clastic or semi-clastic, is separated by a distinct unconformity from the older rocks and called Algonkian—or Archæozoic—or Proterozoic. The latter are described as Archæan, but it should be noted that Dr. Séderholm does not accept the distinction between the Algonkian and Archæan

which has been made in America. For him, Archæan is a term covering only the oldest pre-Cambrian basal beds, with only locally a distinct chronological value. In Southern Finland, each group is divided by an unconformity into two natural sub-groups. The unconformity among Archæan rocks is often obscured by metamorphism and by the intrusion of granite on an extensive scale. The upper formations of the Algonkian series are distinguished as Yotnian, the lower are called Yatulian. The former beds are represented by an olivine-diabase on the western coast in the Björneborg district, and on the eastern side by some gabbros lying to the north of Lake Ladoga. But the most noteworthy member of this series is the remarkable porphyritic granite called *rapakivi*, which occurs in several localities in southern Finland, its most extensive development being along the southern coast from Viborg to Lodovisa. This granite-covered tract stretches inland to the northwards for some distance, and at a moderate estimation extends over some 4,500 square miles. It is a handsome stone capable of receiving a good polish, and it has therefore been extensively used in the erection of the public buildings and monuments of St. Petersburg. The lighter-coloured variety is the more durable. The darker variety has proved unable to resist the severities of the Russian climate. In interiors it lasts well, but exposed to weather it readily disintegrates, and the trouble and annoyance thus caused have given rise to the contemptuous name *rapakivi*, the rotting, or, decaying stone. An excellent example is found in that handsome feature of the northern metropolis, the celebrated Alexander column. This greatest monolith of modern times was originally 102 feet long, exclusive of pedestal and capital. The shaft has now been shortened to 84 feet. It is sadly fissured, and, although carefully repaired with cement, it is considered to be in a perilous condition.

Rapakivi is a hornblende-biotite granite. The characteristic of both varieties is the presence in a coarse-grained aggregate

of phenocrysts of roundish ball-shaped orthoclase never found with crystalloid boundary-faces. The balls range generally from 2 to 3 inches in diameter, but are sometimes larger. Occasionally they contain zonal inclusions of other constituents, and they vary in colour from brownish-red to rose-pink. They are surrounded by a rind of oligoclase of greyish-green tint. The latter is said to be the part affected by the weathering when the orthoclase-nucleus falls out. Hornblende and biotite (var. lepidomelane) are present in equal quantities, and accessories are zircon, magnetite, ilmenite, apatite, and fluor. In the darker variety, the blackish quartz is irregularly bounded, but in the lighter the crystal-faces are well developed. We should also add that Finnish geologists sometimes apply this name *rapakivi* to all holocrystalline granitic rocks of pre-Cambrian age containing phenocrysts of orthoclase, even when the latter are not coated with oligoclase.

The Yatulian diorites, quartzites, and conglomerates are chiefly developed in the north of Finland. The only locality where the writer had the opportunity of observing them was the island of Högland.

Below the Algonkian strata, and separated from them by a marked unconformity wherever they occur together, we find the Archæan beds. The most interesting formations of the upper division of this series are the Bothnian Schists. Above them are granites, sometimes intrusive and sometimes exhibiting a gneissic character; and older than the Bothnian Schists are extensive beds of metamorphic granite, and Dr. Sédérholm's "granitized mica-schists," which may perhaps be more accurately described as finely foliated gneiss. The upper Archæan group is well developed about Tammerfors, the younger beds lying more towards the north. The Bothnian Schists stretch from the Gulf of Bothnia in considerable patches almost due east and west, passing between the granites and the town. In other districts they are said to be represented by uralitic porphyries and gabbros. All these beds rest unconformably upon another

series of granites, diorites, gabbros, schists, and gneiss. Among the pebbles in the Bothnian Conglomerates, there are fragments of syenitic granites and diorites, closely resembling the diorites connected with the older grey granites. The latter do not penetrate the younger schists, but in Lavia they are brecciated at this contact. These Archæan formations in turn overlie an unknown thickness of very ancient granitoid gneiss, which is developed in the district lying to the north of Lake Ladoga, and which, being identified by the Director of the Geological Commission as the most ancient rock in Finland, has been called by him Katarchæan.

The Bothnian Schists of Tammerfors are specially interesting, not only because their original conglomeratic character in certain places can be sometimes clearly traced, frequently in the Tammerfors district being remarkably fresh and distinct, and not unlike certain conglomerates on the western coast of Scotland in appearance, but also because they have been described as fossiliferous. But although one may theoretically admit that conditions during which life was possible probably obtained during a pre-Cambrian period far longer than that covered by the post-Cambrian systems already recognized, it was felt by many members of the International Geological Congress that extreme caution should be exercised in accepting the evidences of organic life at the remote epoch when the Bothnian Schists were being formed, seeing that they are separated from the Cambrian Series by three unconformities and the whole of the important series of Yotnian and Yatulian beds. The grounds on which the Geological Commission claim to have proved the existence of life during the Bothnian period are the not infrequent presence of carbonaceous matter in these phyllites in thin bands, and also lining small cavities, which are claimed as casts of fossils, and traces of structure in this carbon. In order that the members of the Congress might satisfy themselves by actual inspection of these phenomena, arrangements had been made to convey them in two large

barges, roughly but sufficiently prepared for their accommodation, which were dragged by steam-tugs for 10 or 12 miles up Lake Näsijärvi, which lies to the north of Tammerfors. The party arrived at the locality in due course, and with some difficulty effected a landing upon rocks whose natural slipperiness was enhanced by the drizzling rain, for which we found Finland famous. The result might have been foreseen. Many worthy scientists soon lost their footing. Bruised and smarting, some refused further to imperil lives so valuable to human progress and resolutely awaited our return. But the rest of us, after much struggling and scrambling, sometimes on all fours, over slimy rocks and greasy ledges, reached a certain belt of these phyllites, where Dr. Sėderholm pointed to some curious calceolate impressions from 1 to 5 inches long and about 1 to 1½ inches across. They are outlined by a weathered rim of a carbonaceous character, somewhat like graphite. They lie in bands approximately parallel to the slates—evidently of sedimentary origin—in which they occur. Sections have been carefully prepared and examined by the best microscopists of St. Petersburg. Traces of organic structure have been plainly found in this carbonaceous layer, which, be it remembered, seemed, at a liberal guess where we saw it, about 0·01 inch thick. Dr. Sėderholm was convinced of their organic origin and almost sure that they were remains of vegetable organisms. The party made their way back to their barges, some with abraded limbs and aching bones, silenced, if not completely convinced.

It will be remembered that Sir R. I. Murchison reported the occurrence of *Eozoon Canadense* in some schists near Viborg. But no reference was made to it by the Geological Commission.

Subsequently, the party landed on several other islands, and traced the Bothnian Schists from point to point. In some localities, there was unmistakable evidence of their clastic origin, but no fossils were found.

Next day, the party travelled to Kulovesi, which lies an hour or two to the west of Tammerfors. They were then conducted to a cliff in some woods about 1¼ miles from the railway-station, where they were shown an interesting section of Bothnian Conglomerates metamorphosed into a fine-grained gneissic schist, rich in felspar, but with comparatively little mica. Although quite gneissic, the original conglomeratic character could be very plainly traced. In some places, lamination was observable, which was attributed to false bedding in the conglomerate.

It is calculated that the total thickness of the Bothnian Schists and interstratified Conglomerates may be not less than 13,000 to 16,000 feet.

A special train was at the service of the party, and much of our time was spent in it. Many localities were visited, where interesting geological sections occurred, either in railway-cuttings or in the adjacent country, especially such as were considered to establish the views held by the Commission. For example, near Orihvesi, some hours were spent in the examination of a contact between the Bothnian Schists and the granite-rocks. In one portion of the section, the granite contained porphyritic crystals, which did not occur in other parts of it. The members of the Commission held that this granite should be referred to two distinct formations. The porphyritic granite was stated to be older than the schists which were deposited upon it, and which included fragments of it. Another part of the granite, which was non-porphyritic, and was stated to contain tourmaline, is evidently intrusive, and therefore younger than the gneiss in which its veins could be plainly traced. It was maintained that this being a lighter colour and finer grain had no connexion with the older mass. This section was traced for more than a mile, for it was understood that the relative ages of the granites and the gneiss were considered of great importance in determining the age of large masses of granite to the south. There could be no doubt that the non-

porphyritic granite was intrusive, but many of the party thought it by no means clear that the contact between the gneiss and the porphyritic granite was a mechanical one. Superficial observations failed to show any essential difference between the two granites. Even the porphyritic character was variable, and the tourmaline was apparently very local in its development. The so-called included fragments appeared to be parts of veins, and the colour was by no means constant. After an interesting discussion, in which widely diverging opinions were uncompromisingly expressed, most of the geologists present were unable to adapt the official view, as they were of opinion that the granite was all part of the same formation, and that it was intrusive and all younger than the schists.

The Geological Congress inspected many fine specimens of the metalliferous ores of Finland, not only at the museums at Helsingfors and St. Petersburg, but also at various localities visited in the Grand Duchy. At Tammerfors, for example, attention was specially invited to some curious water-worn concretions of iron ore, about 1 inch in diameter, said to be found in abundance in the neighbourhood. The time at the disposal of the party did not permit personal inspection of the mines. But numerous magnetite and spathose iron-ore deposits occur in profusion all over the country. Copper, tin, zinc, lead, and silver ores, auriferous sands and amber are also worked advantageously. Many of these are shown on Prof. Von Moeller's useful map of the ore deposits of Russia in Europe, published in 1878. Perhaps the best known copper and tin ore beds are those of Pitkäranta on the north-eastern shore of Lake Ladogo, but the production of tin is not large. An excellent description of these interesting mines, by Dr. Törnebohm, will be found in the *Geologiska Föreningens i Stockholm Förhandlingar*, 1891, vol. xiii., page 313. That writer's map shows the rapakivi outcrops occurring quite near the ore deposits.

In Southern Finland, the ancient rocks are sometimes covered with glacial clays and sands. Post-glacial beds with

numerous shells such as *Cardium*, *Litorina*, *Mytilus*, *Tellina*, etc., also occur, not infrequently forming terraces, or raised beaches. But the most noteworthy remains of the Quaternary period are long high narrow ridges, chiefly composed of sands and gravels, and distributed widely without apparent reference to present physiographic features. These åsar are similar in some respects to the raers of Norway, the kames of Scotland, and the eskers of Ireland. Sometimes they cannot be satisfactorily distinguished from the arenaceous glacial beds on which they stand, but more often they rest directly upon the ancient rocks. These huge embankments pass in long windings across vast tracts of country. Some of them can be traced right across Finland and into Scandinavia. Occasionally they present striking resemblances to river-courses, coalescing and forming hollows, which are frequently occupied by lakes. Sometimes the åsar are composed of coarse gravel, shingle or earthy detritus. The stones are water-worn, especially at moderate elevations. Erratic blocks are rare in their interiors, although they are not uncommonly scattered over their surfaces. In finer grained beds the stratification is often well marked and false bedding not infrequent. In some cases, the planes of stratification towards the surface more or less correspond to the external slope, which seem to indicate that these åsar are original forms of deposit, and not the result of the erosion of more extended strata. Some of them are 200 feet high, and as the country is gently undulating and well furnished with wood and water, magnificent panoramas are obtained from their highest parts. At the base, they are sometimes 500 to 600 feet across. Tammerfors, the Manchester of Finland, is partly built on an ås, which, forming a natural dam, keeps the waters of Lake Näsijärvi at a sufficient height above the level of the southern lake, Pyhäjärvi, to obtain water-power for all the mills and factories of the town.

No organic remains have been found in åsar, but their external slopes are sometimes covered with an argillaceous deposit containing shells of *Arca*, *Astarte*, *Cyprina*, and *Natica*.

The party had the privilege of hearing many addresses in various languages from eminent geologists, who attempted to explain the origin of the åsar. The theories were very conflicting, but might be grouped as glacial, fluviatile, or marine. The great length, the narrow section, the winding character, and the branching of the åsar present peculiar difficulties. They are clearly to be distinguished from the terminal moraines of glaciers, the latter being unstratified and more limited in extent and lying with their axes at right angles to the glacial striæ; but it was argued that they might have been produced by floods caused by the rapid melting of vast masses of snow and ice. Others suggested that they were the retreating deltas of turbid streams, issuing from the base of glaciers during a period when they were shrinking. Possibly they might indicate the courses of persistent channels in the ice floes, when the country was covered by the glacial sea, along which, for considerable periods of time, icebergs travelled, gradually dropping the solid matter they conveyed. Another explanation was that they marked ancient river-courses. In a country covered with sand, rivers formed their beds, and then deposited coarser materials throughout their length, which resisted the denudation that removed the circumjacent strata. Lastly, they were compared with the submarine banks sometimes formed within the sphere of action of tidal currents sweeping along the shore. Those of us who were not committed to any theory thought that no single one then explained to us could satisfactorily interpret all the phenomena to be accounted for.

At the close of the excursion the party visited a rocky island in the middle of the Gulf of Finland called Högland, to which the writer had already referred. Although only about 6 miles long and less than 2 miles across at its widest part, it bears to Finland a geological relation similar to that which Britain bears to the Continent of Europe, for most of the formations of the mainland are represented upon it. It also offers some fine examples of glacial erosion, and its raised beaches clearly indicate the variations in level of the ancient seas.

The eastern side of the island has a picturesque outline, formed by a series of hills which stretch from north to south, and sometimes raise their summits upwards of 500 feet above sea-level. These highlands are covered with a quartz-porphyry, which our guide, Dr. Wilhelm Ramsay, who had spent some years in working out the geology of Högland, correlated with the rapakivi of the mainland. If so, these strata would be of Yotnian age. They are certainly the youngest of these insular ancient beds. The porphyry generally exhibits a micro-granitic or micro-pegmatitic structure, with large crystals of orthoclase and quartz, but it becomes felsitic and even vitreous near its contact with the older rocks. That it has been raised vertically since its formation we may conclude from the friction-breccias which it contains along the eastern coast of the island.

The quartz-porphyry contains fragments of an interesting rock which Dr. Ramsay called a labrador-porphyrite, or porphyritic diabase. This is an eruptive rock of dark green colour and fine-grained texture. It contains slender, light-coloured felspar laths and much epidote. It underlies the porphyry, and is accompanied by beds of tuff containing fragments of the conglomerate on which it rests.

This conglomerate is a hard sandstone with pebbles of the subjacent older quartzite. It has a slight dip towards the east, and is of about the same age as some fine-grained quartzites by which it appears to be represented in certain localities, as on the west of the rock of Majakallio, and which are changed into hornstone, where overflowed by a labrador-porphyry.

Much more ancient than the above are another series of quartzites and some eurites. These beds have been violently dislocated, folded, contorted, and partly denuded before the deposition of the newer quartzites and conglomerates. At Purjekallio and Somerinvuori, the latter form the tops of the hills, and rest upon the abraded folds of the first-mentioned beds. Owing to this unconformity, the older quartzites and eurites are considered by Dr. Ramsay to be of Yatulian age.

In the north and south of the island, beds of highly-contorted gneiss and crystalline schists are found. In the centre of Högland, these strata appear to be represented by a gabbro, composed of labradorite, partly replaced by saussurite, oligoclase, actinolite, and uralite. These are evidently the most ancient beds in this island, and Dr. Ramsay called them Archæan. They have all been strongly contorted, and sometimes are much folded. A reddish granite has intruded into all of them, and they frequently contain veins from the parent mass, which comes to the surface in the extreme north and south of the island. This granite never penetrates the ancient quartzites, or eurites, or any of the newer rocks. It is therefore considered to be the youngest representative of the Archæan formations of Högland.

Another party devoted their day entirely to the glacial geology of the island. They made a profitable excursion under the direction of Dr. Berghell. The section who accompanied Dr. Ramsay were primarily intent on studying the more ancient rocks, but incidentally many traces of ice-action were seen. All regretted that it was necessary to return to St. Petersburg that night, as the first meeting of the International Geological Congress was held on the following afternoon. But every member of the Finland expedition will always remember the great kindness, courtesy, and generous hospitality with which they were everywhere received during their brief travels in that interesting and ancient country, and they feel that their grateful acknowledgments are specially due to Dr. Sëderholm and Dr. Ramsay and the staff of the Geological Commission.

APPENDIX.—PRE-CAMBRIAN FORMATIONS OF SOUTHERN FINLAND.

	Formations.	Grand Duchy of Finland. Dr. J. J. Sederholm.	Högland Island. Dr. M. W. Ramsay.
ALGONKIAN FORMATIONS.	Yotnian.	Olivine-diabase. Sandstone. Rapakivi. Gabbros.	Quartz-porphyry. Labrador-porphyrite. Conglomerates and quartzites.
	UNCONFORMITY.		
	Yatulian.	Absent in Southern Finland, represented in Northern Finland by quartzites, etc.	Quartzites and eurites.
UNCONFORMITY.			
ARCHÆAN FORMATIONS.	Upper Archæan.	Intrusive granites. Red gneissic granites. Bothnian Schists and Conglomerates. Uralite porphyrites. Schists.	Red intrusive granite. Uralitic-gabbro.
		UNCONFORMITY.	
	Lower Archæan.	Granites, gabbros, etc. Gneiss and crystalline schists.	Gneiss and crystalline schists.
	Katarchæan.	Granites and gneisses of Eastern Finland.	

An Account of a Journey through Russia in the Late Summer and Autumn of 1897.

By L. L. Belinfante, M.Sc.

[Read March 3rd, 1898.]

On Sunday, July 25th, my friend, Charles Palache, and I arrived at Warsaw, the ancient capital of Poland. Encircled by its fifteen forts, it rises on an outlier of Palæocene above the vast Drift-plain which stretches eastward deep into Holy Russia, and westward and northward to the Baltic. Hence it is a 30 hours' journey, some 750 miles, to Moscow, the White-walled City. The track is laid mostly in sand-ballast, and in those hot July days and nights the dust blown up into the railway-cars formed an excellent illustration of a 'loess-deposit,' encrusting hats and coats, and rugs and seats, and everything. The train bore us along through a fairly fertile country, oak, lime, spruce, and pine-forest alternating with rye- and potato-fields. As we near Orsha and Smolensk the landscape assumes a more open and hilly character; but although we watched eagerly for the outcrops of Devonian, Carboniferous, and Jurassic, we failed to discern any. After passing Borodinó, the railway follows the watershed, running parallel to the valley of the Moskva amid the typical scenery of Central Russia: an undulating prairie, with hills, some flat-topped, scattered irregularly about in it, and seamed by small watercourses. All this country was originally a forest-region, but the march of civilization has swept away the forest, and turned the land to arable and pasture.

Moscow, like Rome, is built on seven hills, some of which are about 500 feet above sea-level, the normal level of the river being about 370 feet. On one of these stands the ancient citadel and palace of the Tsars, the historic Kreml. Here the

Lower Glacial Sands rest on Volgian and Jurassic, and these again on Carboniferous Limestone. It is on this limestone that the foundations of the great buildings of Moscow repose: as, for instance, the vast gilt-domed Cathedral of the Saviour and the piles of the bridges. The water-supply of the city is obtained from springs 12 miles away, coming out below the Lower Glacial Sands. The Drift-deposits in this neighbourhood are classified by Nikitin as follows:—

1. Upper Sands seen in small hills.
2. Unstratified reddish-brown Boulder Clay, sandy and marly, containing erratics of crystalline rocks from Finland and Olonetz, and of sedimentary rocks from the intervening country (Carboniferous limestones and cherts).
3. Lower Sands, red and yellow, interbedded with gravels.

The city is paved with pebbles and boulders from the Drift.

South-west of Moscow rise the Vorobievy Gora, or Sparrow Hills, among birch-woods which Peter the Great planted with his own hands. Here may be seen a succession from the Kimmeridgian upwards, through Volgian, Neocomian, Gault and Aptian, to Boulder Clay.

It may be well to explain here that the exact horizon of the Volgian is still a subject of dispute among Russian geologists. Nikitin thinks that it is premature to correlate precisely this formation with any of those of Western Europe, but regards it as equivalent to some of the upper horizons of the Jurassic and lower horizons of the Neocomian. In confirmation of this view he points to the fact that there is a Lower Portlandian fauna in the lower portion of the Volgian, and a Lower Neocomian fauna in the upper portion. Bogoslovsky agrees, on the whole, with Nikitin; but Pavlow does not accept even the term "Volgian," and regards the deposits which are so called as entirely Jurassic.

On July 29th, some of us took part in the excursion down the river to Miachkovo. Here there is a magnificent section of Jurassic resting directly on the Carboniferous Limestone. This limestone is the so-called Moscovian, the middle division of the system in Russia, and is extremely rich in fossils.

On Friday, July 30th, just after sundown, we members of the Geological Congress, left Moscow by the Riazan railway, in a special train composed of 13 corridor-cars, and the next morning found us at Riajsk. Thence we turned due east *via* Morshansk and Penza to Batráki, where on Sunday, August 1st, we set eyes on the Volga, the mightiest river in Europe, for the first time. From Moscow to Riajsk the train had carried us over Volgian, Jurassic, and Carboniferous rocks during the hours of darkness. But with daylight we were running over Boulder Clay and drift-deposits, topped by loess-clays, terrace-clays, etc. (of pluvial and æolian origin). These are covered by the famous *chernoziem* or black earth, rich in humus, lime, and zeolites, and the forest-earth, characterised by siliceous efflorescences. Here, within the memory of living man, the steppes have been converted into arable land, and much of the forest has been cut down. Latterly an Imperial Rescript has put an end to the destruction of forests in Central and Southern Russia: it is now the law that forest-land must always remain forest-land. The scenery reminded my American travelling-companions of their own Far West—it showed the same wide horizons, the same sparseness of habitations. Here and there were belts of woodland: young oak, aspen, birch, and lime. We observed some small salt-lakes in the open prairie. As one approaches the Volga the country becomes more picturesque: ranges of hills show, in the deep ravines which cut through them, a succession of almost horizontal strata. Far to the right gleam the white dusty summits of the Cretaceous ridge of Syzran.

From Batráki a steamer took us a few miles down the Volga to the classic locality of Kashpur. Here the following section was studied:

Cretaceous Marls, with *Inoceramus*, forming the top of the hill,
Aptian,
Neocomian Clays,
Volgian,
Kimmeridgian.

As in so many other Russian localities, and at so many other geological horizons in Russia, the lithological resemblance of the rocks with those of similar age in Great Britain and Western Europe is quite startling. Wandering along the shore at Kashpur, bestrewn with ammonites and belemnites, and looking up to the blue-clay cliffs with their pyritic and septarian nodules, one might almost fancy one's self on the beach of Kimmeridge Bay.

The Volga, after Kashpur, resumes its normal course southward. Above that locality it makes an enormous loop, forming the great peninsula known as the Samarskaia Luka. This originates in a dislocated anticlinal, with a north-and-south axis, cut through by a fault running N.E. and S.W., which throws the Cretaceous down against the Carboniferous. Nikitin supposes that the movement took place about the end of the Oligocene period.

Above Batráki, the railway crosses the Volga over a great bridge nearly a mile long, and estimated at 150 feet above river-level. North of this bridge, we landed to examine the cliffs of more or less dolomitized Carboniferous Limestones impregnated with bitumen, and containing horizons rich in *Fusulina*. On the left bank of the Volga, in the south-eastern bend facing the Samarskaia Luka, is the city of Samara, a busy port and trading-place. We steamed up river to the picturesque Samarskaia Vorota, or Gates of Samara, where the Volga cuts through the great anticlinal—with the Jeguli Hills on the west and the Sok Hills on the east. One passes successively the Permian compact limestone, the cavernous brecciiform limestone, and the Carboniferous Limestones. The Tsarev Kurgan, or Tsar's Tumulus, forms an isolated mass of Carboniferous Limestone on the eastern bank: looking thence across a mile of river, you see the Jegulis covered with pine and oak-forest, scarred by limestone-quarries, and ravined by deep-cut, narrow glens. Here we saw very striking examples of river-terraces, formed by the "terrace-clays."

We spent Tuesday, August 2nd, travelling north-eastward over the Permo-Triassic (Tartarian) and Permian plateau to Ufa. The effects of erosion in a dry continental climate were here especially conspicuous. No vegetation covered the hills, whose bare, wind-swept tops do not even retain the snow in winter. The varied colours of the marls, and sands, and limestones—blue, brown, white, tea-green, and red—enable one to trace from afar the succession of the strata, which have a very low dip or none at all. Low down in the Permian, below the fossiliferous grey marls, limestones, and red sandstones, is a gypsiferous group. The cylinders for the railway-bridge over the Bielaia or White River struck the gypsum after going down through about 50 feet of alluvium. The foundation is insecure, and so well is this known that the pious *mujik* mutters a prayer and crosses himself as the train bears him over the bridge. Elsewhere along the railway, the gypsum-beds give rise to swallow-holes and landslips, which necessitate constant repairs to the embankment and cuttings.

Broadly speaking, the section in this area is as follows, in descending order :—

Chernoziem.
Terrace-clays.
Brackish and freshwater Caspian clays.
Tartarian (Permo-Triassic) variegated marls, clays, and sands.

Permian	Grey marls, limestones, and sandstones.
	Lower Red Rocks.
	Lower Zechstein.
	Gypsum-beds.

Going still north-eastward we entered at Asha the picturesque scenery of the Urals. It was a brilliantly hot day as we trudged along the sand-ballasted track which follows the Sim Valley. Here the landscape reminds one now of the Wye Valley in Monmouthshire, and now of the country about Rübeland in the Harz. On the way to the ironworks of Miniar we passed through a gorge showing the Permo-Carboniferous (Artinskian) limestone, followed by the Upper Carboniferous *Schwagerina*-

limestone, and this is faulted against the shales and sandstones of the lower part of the Middle Devonian: the last-named rocks show wonderful contortions.

We may pause for a moment here, and draw attention to one or two points connected with the general structure of the Urals.

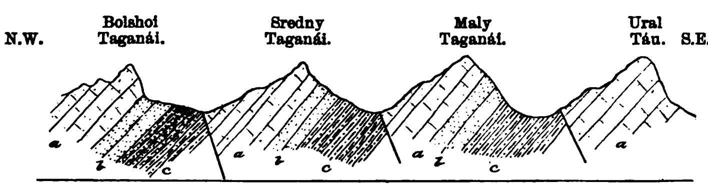
Firstly, as to the river-courses on the western side. These begin by flowing longitudinally, parallel to the main chain, then they suddenly turn westward, and flow through deep gorges cutting across the stratification. Lower down they flow in broad alluvial valleys, changing frequently their bed, and showing well-marked terrace-formations.

Next, as to the succession of the rocks. We have already seen that this is quite regular, from Mesozoic to Devonian. Now the Devonian rests conformably upon the metamorphic schists and crystalline rocks which make up the ridge of the Ural Táu, the watershed between Europe and Asia. It is startling to an English geologist to learn that there is no trace of the early Palæozoics in this great mountain-range, no Silurian, no Ordovician, no Cambrian, no Archæan. One cannot help thinking that a more minute survey may perchance reveal such evidence as will modify the views of the Russian geologists in this respect.

As we advance into the Urals it is noticeable that the Palæozoic folds become more compressed, which is just what we should expect in the case of mountain-building. Beyond Miniar, around the Lake of Simsk, the Artinskian strata are seen resting against the Carboniferous Limestones: these show their nearly vertical bedding in a magnificent crag. Travelling on through the Devonian we came to Viázovaia, whence a 45-mile drive through forest, over rough boulder-strewn tracks, across morasses, up hill and down dale, brought us to the Bakal iron-mines. A very remarkable feature of the landscape hereabouts is formed by the jagged quartzite-ridges which top the hills like the teeth of a comb. Here the dolomitic Devonian Limestones are seen to pass into spathose iron-ore, and this again higher up, near the surface, passes into hæmatite.

From the mines we came back again to the railway at Suleia. In the cuttings betwixt this and Zlatóust, granite is seen veining the dolomite. Our excellent leader Chernyshev persisted in calling this 'rapakivi-granite;' but with all respect to his authority I am bound to say that the hand-specimens which I saw hammered out *in situ* were ordinary coarse-grained granite, showing no similarity whatever to the typical rapakivi of Finland.

At Zlatóust we entered upon the region of metamorphic and igneous rocks, and here we saw in the Kossotur and Urenga Hills an interesting succession of amphibolites, garnetiferous schists, diabases, and gneisses. Despite the unfavourable weather, some of us were enabled to climb to the summit of the Bolshoi Taganái, the highest mountain in this part of the Urals, some 4,000 feet above sea-level, and about 11 miles away from Zlatóust. As you pick your way up the winding track through the forest, you now and then come upon the huge boulder-streams of white quartzite, seen through the trees like great silent waterfalls. If memory serves right, we had to cross five of these. The following section is given in Chernyshev's paper in the *Guide des Excursions*, and explains the structure of the country better than words :—



[*After Chernyshev.*]

a = Quartzites.
b = Friable Sandstones with decomposed felspar.
c = Garnetiferous Mica-schists, sometimes with limestones.

The quartzites of the Great, Middle, and Little Taganái correspond exactly to the Lower Devonian quartzites and grits already met with on the way to Zlatóust, and the underlying mica-schists are regarded by Chernyshev as the metamorphosed representatives of the Lower Devonian shales seen in the westernmost portion of the Urals.

A joürney by road from Zlatóust to the Kussa ironworks enabled us to examine a diabase-sill in contact with the dolomitized Middle Devonian limestones, and next day we reached, on the summit of the Alexandrovskaia Sopka (Ural Táu) the divide between Europe and Asia. The quartzites which go to form this ridge are apparently the same as those of the Taganáis. We threaded our way up the hill amid a chaos of huge quartzite-blocks, with the morning sunlight glinting through the waving birch-branches on the gold-green moss. A splendid panorama met our eyes as we gained the summit. Far eastward, dotted with lakes, and cut by a mighty river, stretched the plains of Asia. West and south rose forest-clad ridges range behind range, and at the utmost horizon the huge dark mass of Irimel reared its cloud-capped summit to meet the frowning sky.

As we came down on to the Siberian steppe, we seemed to return to the scorching heat and dust of the plains of Central Russia. Among the Urals we had experienced rain more or less every day.

The streams on the eastern slope flow away from the axis, towards the great rivers which water the vast plains of Siberia and ultimately debouch into the Arctic. There is no tendency to that parallelism of the water-courses with the mountain-range which we observed on the western side of the Urals.

We spent a little time around Mias and the Ilmen Hills. Here occurs the rock known as miassite (biotite nepheline-syenite), and here is the original locality of the mineral ilmenite. The hills are pitted with little quarries, some of which are still worked for the various minerals got in the miassite and gneiss: sodalite, which forms beautiful blue streaks in the rock; zircon, of which fine crystals were found by some of us; amazon-stone, corundum, etc.

Near Mias, too, we visited the Ilmensky gold placer-deposit. Here below 7 to 15 feet of peat, sand, and clay, is an auriferous

layer about 30 inches thick, consisting of pebbly loam and gravel, resting on 7 feet of gravel, sand, and clay. Below this, borings have revealed talc-schists and serpentine.

At Cheliábinsk we reached the junction where the great Trans-Siberian Railway branches off from the Ural and Perm line. It was our farthest point east: from that place onwards we turned northward, and then westward again over the Urals.

Near Cheliábinsk are the Vonliarlarski gold-mines, where we saw the auriferous quartz-reef splendidly shown in the granite. It appears to be from 2½ to 4 feet thick, and is said to yield 10 to 13 grammes of gold per metric ton.

As the train brought us into Kyshtim there was just enough daylight left to note the splendid outcrop of gneiss in the railway-cutting. Next day, an ascent of Sugomak, a hill chiefly made up of serpentine-rock, enabled us to form some idea of the physiography of the Siberian plateau: the great stretch of Palæocene beds is dotted with innumerable lakelets, and as the eye ranged to the far horizon it was arrested now and then by the gleam of distant water. Looking westward and southward we saw the forest-clad ranges of the Urals, with their chief summits mantled in cloud, a view reminding one of that seen from the Alexandrovskaia Sopka.

Pursuing our journey northward, we reached Yekaterinburg on Monday, August 16th, and from that city we journeyed by a very fair road (as Russian roads go) to the gold-mines of Beresovsk. Here the gold occurs in quartz-veins which cut obliquely through bands of beresite. This rock, which consists of quartz, muscovite, and pyrites, in the new cuttings where we saw it, has as much the appearance of decomposed granite as anything else. On the way to Beresovsk, we stopped at the so-called 'stone tents' of Shartash. These are great rocks of grey granite, split up by horizontal joints into tabular masses: the surface is much rounded by weathering. Some Russian geologists seem disposed to regard their presence as due to human agency, but that is an opinion to which I should be very loth to subscribe.

The road led us along the shore of Lake Shartash, which is fringed by a remarkable moraine. This has evidently been formed by lacustrine ice, as Clerc ingeniously explains. The moraine is now largely overgrown with bushes and young forest.

Another day brought us to the mining-district of which Nijni Tagilsk is the centre. Here we first visited the iron-mines of Mount Vissókaia. In these mines masses of magnetite are associated with distinctly-bedded, quartzless porphyries; these, in places, pass into breccias with enormous garnet-crystals. The orthoclase-rocks decompose into white and pinkish clays, among which blocks of magnetite are seen jutting out.

But to many among us, the chief interest of the district lay in the platinum-mines. On an unspeakably rough road, we drove through magnificent forest, over the watershed, into Europe. As we drew near the goal of our journey we noted many stream-washings where platinum has been got. It is associated with chrome iron-ore in olivine-serpentines, and appears to concentrate in these along the lines of decomposition. The hope that we should find visible platinum *in situ* was disappointed, but the manager of the mines, Mr. Hamilton, very generously gave us a whole series of specimens of platiniferous serpentine and the associated rocks. It was a weird, moonlight drive, over the watershed, through the silent forest, back into Asia again.

Leaving the Demídov estate, we next day made our way to the Crown property of Goroblágodat. On the journey thither we went to Mount Sináia, where certain quarries show very intimate interpenetration of gabbro and diallage-hornblende rock without felspar *(schlieren)*.

Neither space nor time will allow of an adequate description of the great magnetite-mine of Goroblágodat. Suffice it to say that here, as at Mount Vissókaia, the rocks are mainly quartzless porphyries. The ore nearer the surface, being more decomposed, is red, while deeper in it is blue. Druses occur

with excellent crystals of magnetite (octahedra and hexoctahedra). The rocks have a curiously bedded appearance, and narrow bands of limestone (Lower Devonian?) are compressed within the porphyritic rocks, east and south of Blágodat. From the summit of the hill we took our farewell glance at the Siberian plateau.

The railway from Uralskaia station descends in grand sweeps to the European slope. Again we passed over the Devonian Series, the Carboniferous Limestone, and Permo-Carboniferous grits, etc., till at Chusovaia we found ourselves in the Kama river-basin. Here gypsum-deposits are associated with the Permo-Carboniferous; and hence, a week's journey down the Kama and up the Volga, *via* Kazan, to Nijni Novgorod, thence by rail, *via* Moscow and Tver, brought us to the capital of the Russian Empire.

It is not proposed to recapitulate here the proceedings at the Congress. After a nine days' stay at St. Petersburg, pleasantly diversified by excursions to Peterhof and the great Imatra waterfall in Finland, I journeyed again to Moscow and Nijni Novgorod, to join the party who had elected to go down the Volga, under the admirable leadership of Prof. Pavlow.

The tempestuous autumnal weather, of which we had had some taste at St. Petersburg, continued during the first days of our trip down the great river (Sept. 8th to 11th). The gale blew so strong that our steamer dashed into a wooden lighthouse, nearly ran down a barge, and had to be moored in mid-stream during the night. At that period of the year the water is very low, and, despite continual sounding, our steamer frequently struck on a sandbank, off which, after more or less delay, she would generally slide without much damage. But in May, when the waters are up, we are told that the river, at the junction with the Kama, becomes a vast lake, whose shores cannot be discerned from the deck of a steamer.

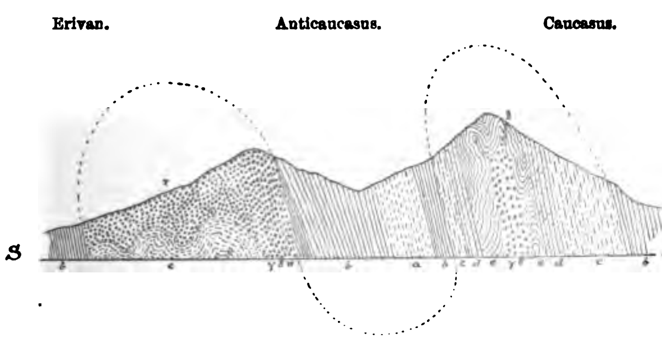
One of the striking features of Volga scenery, along the whole length of the river, with the sole exception of the

Samarskaia Luka, is that the right bank forms a continuous escarpment, a great cliff-line, while the left bank rises gently in low alluvial terraces.

From Nijni Novgorod, for a day and a night's journey down past Kazan, we saw the now familiar red, pink, and grey marls, limestones, and gypsum of the Permo-Triassic (Tartarian) and Permian. Then the Jurassic came in; and at the small village of Goredishché we landed to examine a section showing Kimmeridgian, Portlandian, and Neocomian, with phosphatic sands and conglomerates and numerous fossils. Thence to the contact with the Carboniferous in the Samarskaia Luka, the rocks are all Neocomian and Cretaceous. Reference has already been made to the section at Kashpur.

Below Volsk there is a great development of Danian; then come the equivalents of the Thanet Sand, and at Sarátov we reach Black Sea-level (the Caspian, into which the Volga pours its waters, is some 70 feet below ordinary sea-level). Here we made an excursion to the Lyssaia Hill, where the Cretaceous marls and sandstones are capped by the Palæocene of the Syzran and Sarátov Series. Farther down, in the white cliffs of Trubino, appear the Cenomanian sands, crowned by *Inoceramus*-chalk. Higher and higher beds come in, till the picturesque pinnacled cliffs are formed of Tertiaries alone: their form and mode of denudation reminding one vividly of the 'chines' of Bournemouth and the Isle of Wight. In the Eocene sands below Kamyshin, huge elliptical concretions of calcareous grit (*karavaï*) appear, not unlike our sarsens. At Tsaritsin the Volga suddenly leaves the Tertiary cliff against which are banked the terraces of post-Tertiary Aralo-Caspian clays, and making a sweep south-eastward, flows through vast steppes, built up of these Aralo-Caspian deposits, past Buddhist temples and Kalmuk settlements, down to the far-famed city of Astrakhan. Its white-walled Kreml, the gilt cupolas of its lofty cathedral, tower above a forest of masts. East and West meet in its markets, and jostle each other in its streets; there is

some of the Oriental colour, and a little of the European polish. An overcrowded steamer took us down the maze of waterways through the delta, where flocks of pelicans were sunning themselves on the sandbanks, and Kirghiz fishermen were mending their nets in front of their tattered tents. A day, a night, and half another day brought us across the storm-swept Caspian to Petrovsk, on the Daghestan coast. Here we visited the fishery-establishments, redolent of sturgeon and the various species of Caspian herring. Then the railway, running westward along the northern base of the Caucasus, brought us, on a beautiful September day, into Vladikavkaz. This place commands the northern outlet of the great Georgian military road, one of the few really good roads that we saw in Russia. It runs southward for 120 miles right through the Caucasus range, over the watershed into Asia Minor, to the old capital of Georgia, Tiflis. The following generalized section, taken from the *Guide des Excursions*, illustrates the structure of the country over which we travelled, from Vladikavkaz to Tiflis and afterwards to Erivan, on the Armenian plateau.



a = Neogene.
b = Palæocene.
c = Upper Jurassic and Cretaceous.
d = Lias.
e = Contorted Palæozoic Schists.
γ, δ = Granite and Diabase.
π = Porphyries.
τ = Trachytes and Andesites.

Along the Georgian road we saw the folding of the Upper Jurassic and its faulting against the Lias splendidly shown, and as we penetrated farther into the core of the range the

Jurassic was succeeded by Palæozoic schists and grits, and these were again followed by the granites and gneisses of the great gorge of Dariel. In this narrow pass there is barely space for the foaming waters of the Terek, and the roadway: it is structurally the very core of the Caucasus. As the pass broadens out again, we come within sight of the great snow-capped peak of Kasbek (17,000 feet), a recent andesitic cone.

Leaving the Terek Valley, with its morainic terraces, and its big glacial boulders, a few of us walked by paths winding up high escarpments of Palæozoic slates, traversed by 'greenstone-dykes,' to the Devdorak glacier, which is an advancing one, fed by the snow-fields of Kasbek. The weather was brilliant in the extreme, and such sunsets and sunrises as we saw from these high altitudes remain for ever pictured in the mind's eye. On resuming again our journey along the military road to Tiflis, we constantly met with magnificent masses of andesitic lava, showing the most perfect prismatic structure. Here, too, we saw the morainic deposits capped by flows of andesite. It was late on the second night out from Vladikavkaz when we crossed the Krestovaya Gora (7,600 feet) into Asia, and on the third night we reached Tiflis. Thence the railway runs along the Neogene and Palæocene plateau to the oil-fields of Baku.

Hereabouts one has a glimpse of true desert scenery; bare, rocky hills glaring in the sun, sandy, stone-strewn plains, with no sign of vegetation except some very small thorny scrub. The Caspian was in gentler mood than when we had last seen it, and through the dust-haze and petroleum-refinery smoke its blue waters sparkled refreshingly in the sunlight. At Baku, as everywhere in Russia, profuse hospitality was shown to the members of the Congress, but many of us found the weather too hot to endure with equanimity four banquets in two days.

Those of us who were among the favoured few whom Lœwinson-Lessing, our most courteous leader and friend, took with him to Ararat, left the petroleum city on Saturday,

September 25th. Thence, duly guarded by escorts of Chapárs, we journeyed two days by road over the Anticaucasus, out of fertile, fruitful Georgia, on to the volcanic plateau of Armenia. Suddenly, like a dream, floating above the clouds 60 miles off, in a bee-line, rose the snow-capped peaks of the greater and lesser Ararat. Stopping to examine some fine exposures of obsidian, we reached Erivan on the Tuesday evening. Nor did we rest there but drove on, across the Araxes, to a posting-station, whence saddle-horses took us next day up to the Sirdar Bulakh Pass, between the two peaks. On September 30th, we made the ascent of Little Ararat, a straight cinder-slope, 5,000 feet up from the pass, 12,500 feet above sea-level. The andesitic blocks of the summit are all scarred and seamed with fulgurites. Looking southward into Turkey, one sees great black crags of lava, with ridge behind ridge of barren mountain. Eastward, far into Persia, the same landscape meets the eye, relieved, however, by beautiful blue lakes.

Here the foregoing desultory account of this journey may appropriately end, although one is tempted to descant on the beautiful dreamy days and soft moonlight nights spent in journeying up the Black Sea from Batum to the vine-clad shores of the Crimea. To the Russian geologists in particular, and to the Russian people generally, all those who participated in the International Geological Congress feel that they owe a deep debt of gratitude. No words can do justice to the unfailing courtesy, the extreme friendliness, the generous hospitality, which met us at every turn.

A Diary of Charles I.'s Detention in Newcastle, May 13, 1644—Feb. 3, 1645.*

By C. S. Terry, M.A.

Newcastle played an important part in the Civil Wars, whether as the backbone of the Northern Association, as the stronghold of the Scots in 1640-41, or as the scene of the negotiations which attended Charles's virtual imprisonment there from May, 1646, to February, 1647. Contemporary accounts of the town bear almost enthusiastic testimony to its beauty and wealth. Sir William Brereton in 1639 found it 'beyond all compare the fairest and richest town in England, inferior for wealth and building to no city save London and Bristow;' the 'revenues belonging unto it (as I was informed), at least £5,000 or £6,000 a year. . . . . There is every day a market here kept . . . . Tuesday and Saturday a mighty market.' Speed in 1646 described it as 'the very eye of all Townes in the Countie, which doth furnish the wants of forraine Countries with her plentie.' To Ambrose Barnes, a Newcastle apprentice during Charles's residence in the town in 1646-47, it was 'the emporium of the north for merchandise of all sorts.' Among its chief features were its walls which enclosed but a fraction of the modern town, which quaint William Lithgow found 'a great deale stronger than those of Yorke, and not unlyke to the walles of Avineon, but especially of Jerusalem.' Of its coal trade, 'the chiefest commoditie that enricheth the Countie' writes Speed, 'are those stones *Linthancrates*, which we call sea-coales, whereof there is such plentie and abundance digg'd up, as they doe not only returne a great gaine to the inhabitants, but procure also much pleasure

* This paper is no more than an attempt to construct a diary of Charles's detention in Newcastle from May 13, 1646 to February 3, 1647. The subject is more fully considered and authorities indicated in the writer's contribution to volume 21 of the *Archæologia Æliana.*

and profit to others.' Grey, however, in 1649, gives a by no means encouraging report, 'I can remember one, of many, that raysed his estate by coal-trade; many I remember that hath wasted great estates. . . . . Master Beaumont adventured into our mines with his thirty thousand pounds: who brought with him many rare engines not known then in these parts; as the art to boore with iron rods to try the deepnesse and thicknesse of the coale; rare engines to draw water out of the pits; waggons with one horse to carry downe coales from the pits to the stathes, to the river, etc. Within a few yeares, he consumed all his money, and rode home upon a light horse.'

The town, the residential portion of it at least, was built upon the steep bank which ran down precipitously from the eminence on which the Norman Keep and the Churches of St. Nicholas' and All Saints' stand, to the river, while somewhat further to the north, on the overlooking table-land, stood the Church of St. John, in the parish of which, hard by the site of the present Central Station, certain of the northern nobility had their town mansions, and yet further north, just within the protecting north-west corner of the wall, the last of the old Churches, St. Andrew's. In the opposite north-eastern corner of the town stood the house called on Speed's map of 1610 'the Newe House,' and later known as Anderson Place, surrounded by grounds which almost filled the northern breadth of the town, in which at different times the Governor and General Leven had their residence, and in 1646-47 Charles held his court. Across the river, from the Sandhill to the foot of the Bottle Bank in Gateshead, stretched the wooden bridge picturesquely edged with gabled houses and guarded by gateway and portcullis, 'one of the finest bridges I have ever met with in England,' says Brereton of it, standing on the site of Hadrian's Ælian Bridge from which in Roman times the town took its name.

Charles entered Newcastle about five o'clock on the afternoon of Wednesday, May 13th. By order of Sir John Lumsden, the

Governor, musketeers and pikemen lined the roads from Gateshead to the house assigned to the King and his attendants. He was received virtually as a prisoner, as appears from the following account by an eye-witness. 'Some that attended upon his Majestie rid before all bare. Then his Majestie marched with the Generall and some other Scottish officers, divers of whom also that were near the King rid bare. There went none out of the Towne of New-Castle to meet his Majestie, neither the Scottish Lords that were in the Towne, nor the Deputy-Mayor thereof; nor any other, either inhabitant or other. His Majestie was not received in triumph (as some would have had it to be done), nor did they, in any solemne manner, take notice of his Majestie. The King rid in a sad coloured plaine suite, and alighted at the General's quarters (now the court). There were no guns discharged, neither by land, nor by water, by way of triumph. There was no acclamation by shooting of muskets, sounding of trumpets, or beating of drums, and yet there were both kettle-drums and trumpets good store in New-Castle; yet were they so far from any publike way of triumph, that they did not sound or beat so much, as when one troop of Scottish horse march into New-Castle.' There was no great concourse of people in the streets, and only as he entered the gates of the court was a slight demonstration made by the adherents of the Parliament.

Particular precautions were taken to secure the King. Proclamation was made that though he was present, yet all were to yield obedience to the ordinances of Parliament. A guard was stationed at the court, and two burgesses were appointed to watch at each of the town's gates, to prevent the ingress of any known adherents of Charles. Several of his attendants were discharged soon after his arrival.

Charles's letters to his wife fully confirm the severity which the above details suggest. On May 20th, he complains of the 'barbarous usage' he had received since his surrender to the Scots. On May 28th, he describes himself as a prisoner:

'I must think myself so, since I cannot call for any of my old servants, nor chuse any new without leave, and that all my friends are forbidden by proclamation to see me;' and again, on June 3rd, 'know that none are suffered to come about me but fools or knaves (all having at least a tincture of falshood), every day never wanting new vexations, of which my publick devotions . . . . are not the least.'

The house allotted to Charles, known in later times as Anderson Place, was then the residence of Sir Francis Liddell. Built originally by Robert Anderson, a wealthy Newcastle merchant, the house and its grounds almost filled the northern breadth of the town. Latterly the house had been used as the residence of both General Leven and the Governor Lumsden, and while Leven continued throughout Charles's visit to live there, Charles seems to have taken the apartments vacated for him by Lumsden. Very little information is obtainable regarding the arrangements made for his accommodation in this improvised court. Lord Lanark acted as his secretary, and the Earl of Dunfermline was in attendance in his bed-chamber. Fifteen dishes were served at his table; the town was at part of the expense of providing him with coals, and until his attempted escape in December he and his suite had full liberty to play golf on the links in the Shield Field outside the walls.

Charles had barely alighted at the court on the afternoon of May 13th, before he was waited on by Lord Callander, Lord Lanark, and Lord Balmerinoch. Callander presented a petition begging him to sanction the Covenant, but Charles diplomatically urged scruples of conscience, and desired that Alexander Henderson, one of the most eminent of the Scottish divines, might be sent for to discuss them with him. The nobles next required that Montrose should receive orders to lay down his arms, at which 'the King fell into a violent passion,' and upon their refusal to grant him the attendance of forty of his own attendants, swore he would 'neither eat nor drink untill he had them.' He was informed curtly that 'he might take his

choice.' In face of their refusal, however, Charles found means to summon certain of his old officers at Newark and, either as the result of his invitation, or merely as the consequence of his presence in the town, a large number of his adherents made their appearance. Leven indeed found it necessary, on Saturday the 16th, to issue a proclamation forbidding the resort of 'Papists' and 'Malignants' to Newcastle, and the proclamation was so far effective that on May 26th it was reported that no royalists were to be found either at court or in the town.

On the 15th, Charles received a further petition from the Scottish nobles, and on the 19th replied to it, undertaking to disband the forces called together in his name in both kingdoms, to recall commissions given to any at sea, and generally expressed his desire to submit himself to the counsel of his Parliaments. On the 20th, he repeated his undertaking in a proclamation to the Scottish nation. The Scots, however, were determined to make the fullest use of the opportunity which their possession of the King gave them. On May 23rd, Loudon, the Lord Chancellor, arrived in Newcastle, and held several interviews with Charles. By some it was reported that the King was satisfied with the progress of affairs, others declared that he was so harassed that he had lost his appetite and at times refused to eat.

Meanwhile, on Saturday, May 16th, considerable excitement had been caused in Newcastle by the escape of Jack Ashburnham, who, with Dr. Michael Hudson, Charles's chaplain, had accompanied the King in his flight from Oxford, and had arrived at Newcastle with him.

On May 15th, Charles had written a short letter to the Queen, commending her for further information to Jack Ashburnham, whom he intended to entrust with his first communication to her since his surrender to the Scots. In order that Ashburnham might escape the vigilance of the guards, it was arranged that he should accept an invitation to dine with Sir Henry Gibbs, at Jarrow, on the 16th. On that

morning, about six or seven o'clock, Ashburnham and Gibbs left the court, and proceeded to the shop of John Dobson, a haberdasher, where Ashburnham purchased a 'Monmouth' cap. From the shop, Gibbs and Ashburnham went to Montreuil's lodgings, and after a conversation with Hudson left the town. On the same afternoon Hudson was on his way across the Tyne Bridge to deliver a message from Charles to Ashburnham at Jarrow when he was apprehended by the Deputy Mayor. After some negotiations between the Mayor and the King, Hudson was sent away from the town early on the morning of the 17th. Ashburnham, some four days later, succeeded in getting a passage on a boat bound for Holland.

On the same day, Sunday the 17th, Charles listened to a sermon from Mr. Douglas one of the Scottish ministers, who 'spake home to him, and advised him to dispose his spirit to peace and unity.' On the following Thursday (May 21st) he visited Tynemouth Castle, attended by Lords Lothian, Dunfermline, and Balmerinoch, proceeding down the river in his barge, dining at the Castle, and returning by land.

On Friday, May 29th, Charles addressed the first of a remarkable series of letters to Alexander Henderson, who had arrived in Newcastle on the 16th. Charles had at first proposed that the controversy should be conducted between Henderson and a committee of ministers. Henderson objecting, Charles suggested a convention of ministers. Henderson replied that such a method had never proved effectual, and asked Charles to conduct the controversy himself by correspondence. Charles consented, and gave Henderson permission to write 'a full yet modest expression of his motives.' In his first letter, on May 29th, Charles exactly expressed the nature of the English Reformation, and his position towards the demands which Henderson and his party made upon him. 'No one thing,' he writes, 'mad me more reverence the Reformation of My Mother, the Church of England, than that it was done (according to the Apostle's defence, Acts 24, 18) neither with

multitude, nor with tumult, but legally and orderly; and by those whom I conceive to have onely the reforming power.' From this position Henderson's arguments did not dislodge the King. The correspondence proceeded leisurely throughout June and July, and closed with a letter from Charles on July 16th, in which, behind a compliment, the King expressed how little Henderson's arguments had shaken him; 'for example,' he wrote 'I think you, for the present, the best preacher in Newcastle; yet I believe you may err, and possibly a better preacher may come; but till then, must retain my opinion.' At the commencement of August, Henderson's health, which had shown signs of weakness as early as June, completely broke down, and he discontinued his attendance upon Charles. On August 4th, Baillie wrote to him, 'your sickness has much grieved my heart . . . . We never had so much need of you as now . . . . the King's madness has confounded us all; we know not what to do, nor what to say, We know well the weight that lies on your heart. I fear this be the fountain of your disease.' On August 7th, writing to Mr. Spang, Baillie adds, 'Mr. Henderson is dying, most of heart-break, at Newcastle.' Some three days later (August 10th), Henderson left by sea for Edinburgh, and Charles visited him on his departure. On August 19th, he died at Edinburgh.

Meanwhile throughout June the Scots had been pressing the Covenant upon Charles. On June 3rd, he wrote to the Queen complaining of the 'vexations' to which he was being subjected, especially in his devotions; and again, on June 10th, 'I never knew what it was to be barbarously treated before . . . . all the comfort that I have is in thy love and a clear conscience.' His state of mind as revealed in these letters is perhaps the only explanation of an extraordinary proposition, communicated to the Scottish Commission on June 5th, for the partial recognition of Presbyterianism in England. Whatever seriousness there was in Charles's suggestion, it gave considerable satisfaction in Newcastle, where he

was reported as being more amenable to argument than he so far had been. The Scots, therefore, plied the King more vigorously. About the 15th, the whole committee, on their knees and in tears, besought him to yield. Charles also was moved to tears and retired to his bed-chamber, but even there some of the deputation followed him and renewed their request. With that experience in his mind, Charles wrote to the Queen on June 17th, 'I hope God hath sent me hither for the last punishment that he will inflict upon me for my sins, for assuredly no honest man can prosper in these people's company.' The importunity of the Scots had already, on June 10th, led him to suggest to Parliament his return to London, and on the 24th, he wrote on the same matter to the Queen, 'I think my proposing to go to London, if I may be there with safety, will be the best put off, if—which I believe to be better—I cannot find a way to come to thee.'

On the 26th, Charles was attacked from another quarter. Leven and about one hundred of the Scottish officers presented a petition, in which, while they vindicated the army from any suggestion of collusion with the King in his recent surrender to them, they begged him to conform to the wishes of his Parliaments. Charles replied in a few sentences on the 27th, expressing his desire for a speedy accommodation, when it would be his care 'to finde out some meanes of honourable employment for so many gallant men as are imployed in the Armie.'

The Commissioners and the army had both done their utmost to move Charles. It was now the turn of the Scotch Assembly. On Saturday, June 27th, Mr. Cant, Mr. Douglas, Mr. Robert Blair, Mr. Andrew Fairfold, and other ministers arrived from Edinburgh. They came in a severe mood. The letters which they brought from the Assembly were not to be delivered until they had had an opportunity of preaching before the King. If their sermons failed to move him, then they were to 'let His Majesty know what the Church censure

is.' On Sunday, July 5th, Mr. Cant preached to the King. His very text was minatory, nor was his sermon less plain and direct. 'Thou piece of clay,' he said more than once, addressing the King, 'where thou sittest, think of thy death, resurrection, judgment, eternity.' Charles listened uneasily to the preacher's denunciations, while others of his auditors were dissolved in tears. Cant, indeed, fully justified the eulogy passed upon him and his fellow-preachers in Newcastle by a contemporary news-letter; 'they court him not in their sermons, nor lay pillowes under his elbowes, as too many of the court divines have done, no, they speake truth with power.' After the sermon Charles was courageous enough to invite Cant, Blair, and the other ministers to confer with him on 'a case of conscience which he would put to them.'

Meanwhile, for some time past, the English Parliament had been debating the terms of proposals 'for a safe and well grounded peace' to be sent to the King. On July 8th, Commissioners of both Houses had received instructions to proceed to Newcastle, 'or at such other place as you shall find the King,' to present to him their nineteen Propositions. The Commissioners reached Newcastle on Thursday, July 23rd, and in the course of the evening they received notice from Charles that he would receive them on the following afternoon.

On that day, about two o'clock in the afternoon, the Commissioners, accompanied by Loudon and Argyle, presented themselves at the court. They were ushered into one of the larger rooms which served as an audience chamber. Charles was awaiting them at the head of a long table and invited them to follow him to another room. His first enquiry was as to their powers to treat with him. Learning that they were empowered only to receive his answer and to communicate it to Parliament, he remarked that 'saving the honour of the business, an honest trumpeter might have done as much.' He listened attentively, however, to the Propositions, and at the close of the reading addressed the Commissioners: 'Gentlemen, I hope

you do not expect a very speedy answer, because the business is of high concernment.' The Earl of Pembroke replied that their stay in the town was limited to ten days from their arrival, and Charles, undertaking to give his answer in convenient time, dismissed them.

On the same day the Commissioners heard a sermon from Samuel Kem, one of the navy chaplains, upon the significant text, 'We certify the king that, if this city be builded again, and the walls thereof set up, by this means thou shalt have no portion on this side the river.' On the Sunday following, July 26th, Stephen Marshall preached before the King. Charles, remembering his experience at the hands of Mr. Cant, sent for Marshall twice and thanked him for a sermon which was 'peaceable and not personal.' On Monday, the 27th, the Commissioners again waited upon the King. His answer, however, was not prepared, and hearing nothing further from him they presented themselves on Saturday, August 1st. At that interview he gave them a verbal answer of so unsatisfactory a nature that they begged for a final audience the next morning. On this occasion Charles gave them his answer in writing. It was short; made no endeavour to deal with the matter at issue; commented on the fact that Parliament had taken twice so many months to prepare the Propositions as days were allowed him for their consideration, and suggested his return to London to debate the matter at length. The Commissioners refused to take such an answer, and Charles commanded them to withdraw to reconsider their refusal. After nearly an hour's debate they returned and expressed their willingness to take his written reply, though they could not regard it as an answer to the proposals which they had been commissioned to put before him. On the same afternoon they despatched a letter to Parliament giving an account of their proceedings. They were fearful, they wrote, lest Parliament should not be satisfied with the King's reply, and therefore they forbore from sending it. They were, however,

returning to London with all convenient speed. Early the following morning, Monday, August 3rd, they left Newcastle, Leven, Lumsden, Argyle, Dunfermline, and the officers of the garrison accompanying them as far as Durham. A week later, on Monday, August 10th, they reached London, and on the 12th received the thanks of the House.

Their departure from Newcastle was followed by that of the Scotch nobility to attend the Convention of Estates at Edinburgh. Henderson, too, had left Newcastle, and Charles was allowed some leisure, which was not broken until the return of the Scottish lords in September. The narrative of his visit becomes at this point considerably disconnected. He does not appear to have abandoned his hope of leaving Newcastle. Both in the first and second weeks of August it was announced in London that he had no longer any wish to stay in that town, though a letter of Sir Robert Murray, on August 8th, speaks of him as being in good spirits over a game of chess. In the fourth week of the month it was reported with some trepidation in London that he was making overtures to some of the nobles in the town, and seeking to ingratiate himself with the soldiery who by now, even the King's special guard, were in a ragged condition. On the 31st, however, he expresses to his wife the view, that only by stirring up France to action on his behalf would there be any 'life in his business.' Newcastle, however, continued to be the Mecca of the King's party, and on August 3rd Leven had been compelled to issue a further proclamation forbidding them from coming near the court.

In September, a further visit from the Scotch Commissioners put an end to Charles's brief holiday. On Wednesday, September 2nd, he went down the river to Tynemouth Castle, and on the following day took some recreation at golf. On Friday, the 4th, the Commissioners arrived, and on the 5th asked for an audience. Sunday, the 6th, was given up to the usual sermons, but on Tuesday, the 8th, and probably on the intervening Monday, on which day Charles complained to the

Queen of having been 'freshly and barbarously assaulted from Scotland,' the Commissioners were with the King. Charles adopted the method already employed with the English deputation in July, and questioned their power to treat with him. On the 9th, the Commissioners assumed a more determined tone. Hamilton, Lanark, and Callander pressed him to accept the Propositions. Their arguments failing to make any impression, Hamilton at length spoke of a further communication which he was bidden to make. Charles at once rose, declared that he knew well the purport of the message, and closed the interview. The nature of this undelivered message affected Charles's wonted equanimity. He appeared to be considerably perturbed in his mind, and had almost abandoned golf and exercise. On Thursday, the 10th, in answer to his invitation, Mr. Blair, Mr. Douglas, the uncompromising Mr. Cant and others, representing the committee of the Scottish Kirk, were admitted to an audience. Charles remained unmoved by the arguments addressed to him, and at length Mr. Cant losing patience exclaimed, 'Sir, I wish I may not say to your Majesty as the Prophet said to Amasiah, 'Refuse not counsel lest God harden thy heart to destruction.' Charles rejoined that Mr. Cant was no prophet but, deftly waiving discussion of this personal matter, Cant replied that, whether he were a prophet or not, it was still permissible for him to recall to the King's notice the warning to Amasiah. On the 11th, the Commissioners conferred with Charles 'in point of honour,' and he undertook to give his answer on the 15th. It proved to be of an indefinite nature. He neither accepted nor declined the propositions put to him, but once more expressed his desire to be allowed to proceed to London, where he would be in a better position to discuss the matter. By the 21st, the Commissioners had left Newcastle, and were soon followed by the other Scottish nobility in the town.

The patience of the English Parliament was at length exhausted. The nature of Charles's reply to the Scotch Com-

missioners was known in London on September 21st. On the following day, a resolution passed for the conversion of the Propositions into ordinances of Parliament, and on the 24th a Committee of both Houses was appointed to treat with the Scots for the disposal of the King. In Scotland equally the futility of any further attempt to deal directly with Charles was recognised, and the Convention of Estates in Edinburgh was already debating the measures necessary for the evacuation of the north of England and the surrender of the King. Charles on his part equally recognised that his relations with both Parliaments had reached a crisis. On September 16th, he wrote to his daughter in Holland, requesting her to send a ship to the Tyne, ostensibly that he might communicate with his wife, actually, as appeared in December, that he might have the means of escape should such a course appear advisable. On the 26th he expressed his views on that possibility candidly to Hamilton, 'I must tell you (and 'tis so far from a secret that I desire every one should know it . . .) that I will not be left in England when the army retires.' He explains his fears: 'Those at London think to get me into their hands, by telling our countrymen that they do not intend to make me a prisoner. O, no, by no means! but only to give me an honourable guard, forsooth.' At the same time, Charles was still careful to represent himself as amenable to reason and argument. On Sunday, the 20th, and again on the 22nd, Blair was admitted to confer with him on the threadworn subjects of Episcopacy and the Covenant.

There are but scattered references to Charles and his surroundings in Newcastle throughout October. On the 3rd, he wrote to the Queen complaining that he was 'generally condemned of wilfulness,' and again on the 26th, to explain the impossibility of his accepting the terms hitherto proposed to him. 'The absolute establishing of a Presbyterian government,' he explains to her, 'would make me but a titular King . . . For thou must understand that (which I find abso-

lutely mistaken by you all in France) the difference between the two governments (Episcopal and Presbyterian) is one of the least disputes now among us, even in point of religion ; for they intend to take away all the ecclesiastical power of government from the Crown. Moreover, they will introduce that doctrine which teaches rebellion to be lawful, and that the supreme power is in the people, to whom kings (as they say) ought to give account, and be corrected when they do amiss.' At the beginning of the month, Montreuil returned to Newcastle, and with Bellièvre was busily engaged with the King. A few days later Charles received a visit from Davenant the poet, who had come from France on the Queen's behalf. About the same time he had an unusual experience. While playing golf on the Shieldfield links, a woman, apparently out of her mind, interrupted his game by loudly declaring that it would be better for him to be with his Parliament in London. She refused to be silenced and added that, if those who attended the King loved him as they ought, they would long since have given him the same advice. On the 15th, Charles was again on the links, while on the 21st, a fast day, 'the hearts of divers honest men' were 'much sadded' in that golf was played in the grounds of the Court.

Throughout November Charles was largely engaged in preparing matter for, and supervising the work of, the printer Stephen Bulkley, whose arrival in Newcastle and his doings there furnished the news-sheets with welcome material in their weekly chronicle of the otherwise uneventful course of affairs at the court. Charles, it would appear, contemplated if not a pamphlet war with his Parliament, at least a wider dissemination among his subjects of such literature as was likely to influence them on his behalf. The consequences of his refusal to accept the terms propounded by Parliament he thoroughly understood. On November 1st, he wrote to the Queen, 'they . . . . tell me, from London, that they will neither declare against monarchy nor my posterity, but merely against my

person.' From his wife he received little sympathy. On the 7th he writes to her, complaining that 'I am so condemned of thee as that my rigidness will be the ruin of all that is dear to me.' Again, on the 28th, he is much perturbed at her threat to 'meddle no more' with his affairs. Already, however, he possessed the means of conducting his 'business' by methods in which the Queen's help was not possible. About the end of the first week in November, there arrived from York a young printer named Stephen Bulkley. He had been summoned to Newcastle by Charles, and his arrival aroused vehement protest and opposition in the town. The attitude of the authorities was so menacing that Charles was compelled to give Bulkley and his press shelter in the court itself. After a few days, Bulkley ventured to set up his press outside the precincts of the court, in lodgings which gave easy access to the King's apartments. Here a further attempt was made to arrest him, from which Bulkley escaped only by timely flight to the King's presence. Thereafter he appears to have been unmolested. On the 28th, he was busily engaged in printing off the first of his Newcastle pamphlets—'An Answer sent to the Ecclesiastical Assembly at London.' The title-page asserted that it was the work of John Diodate, of Geneva, a stout Protestant, the author of French and Italian versions of the Bible. Its appearance caused considerable surprise and even consternation, and drew, in addition to vehement disclaimers of the attributed authorship, a vigorous answer in *A reply to a letter printed at Newcastle.*

About the middle of the month, a Dutch man-of-war entered the Tyne. She had come over in answer to Charles's request conveyed to his daughter on September 16th. She was placed absolutely at Charles's disposal, and her commander brought despatches from the Prince of Orange. As events proved, the visit of the Dutch ship was clearly connected with some vague design of escape on Charles's part. He had already broached the project to his wife, without, however, gaining her assent. On

December 5th he wrote to her, undertaking not to take such a step until it had become clear that he could not count upon any further support from the Scots. On the same date, however, an order was made in the English Parliament to send down the first instalment of the indemnity due to the Scots, and the knowledge that the withdrawal of the Scots and his own surrender to the English Parliament was now only a question of weeks undoubtedly spurred on Charles and his adherents to take the course already suggested to the Queen. Throughout December the loyalists were flocking into Newcastle. On the 3rd, Leven and Lumsden found it necessary to order their withdrawal from the town and surrounding district. Everything points to some fear on the part of the authorities that an attempt to remove Charles was imminent. The captain of the Dutch ship was examined, and the governor of Tynemouth Castle was warned to be on his guard.

On Sunday the 6th, Charles heard a sermon from a Scotch preacher who outvied even Mr Cant in the energy and violence of his oratory. At the end of his discourse, the preacher called upon the congregation to join with him in the 52nd Psalm, 'Why dost thou, tyrant, boast thyself, Thy wicked works to praise.' Charles, with considerable readiness, called for the 56th Psalm, 'Have mercy, Lord, on me, I pray, For man would me devour.' The congregation, already impressed unfavourably by the preacher's methods, followed the King, some in their excitement singing who had not done so for years.

On the 10th, another man-of war, from Dunkirk, entered the river, and in spite of the recent proclamation the loyalists still remained in large numbers in Gateshead and the country round. Five more Dutch men-of-war were expected, while the Dunkirk ship was reported as being the first of others which would arrive shortly. It is impossible to regard these movements as being other than part of a pre-arranged scheme. Towards the end of the month it was ripe for execution. On

the 24th, Tobias Peaker, groom of the Privy chamber, was sent by Will Murray with a message to the Dutch captain at the Peacock Inn. The captain returned with Peaker to the court, and later in the day received £100 as an earnest of the reward which would be paid him in the event of his successfully accomplishing Charles's escape. On the evening of the same day Peaker once more was sent to the captain to enquire if the wind was fair, and if the ship was prepared to leave the river in face of any opposition that might be encountered from the garrison at Tynemouth. The captain expressed himself as quite ready, though he would prefer to go out of the river on the day tide. With this information Peaker returned from Tynemouth the next morning. The escape was planned for that evening (December 25th). What actually took place can with difficulty be made out. Charles remained up till late at night ready to make his escape from the court in disguise, and Murray's actions were so suspicious that the attention of Leven was called to him. Some one, Murray probably, in grey clothes, was observed in the grounds surrounding the court, and an attempt of some kind was made to pass through one of the sallyports in the town walls. The guards were vigilant, however, and the project was for the time abandoned. Suspicion had already fallen upon the Dutch captain. On Thursday, the 31st, therefore, Peaker was despatched to Hartlepool with a letter to the governor, Lieut.-Colonel Douglas. He had not proceeded further, however, than half-a-mile beyond Gateshead when, as he expressed himself in his evidence given later in York, he began to ponder 'the consequents of that business,' and 'not being willing to be accessory to an action which might prove so prejudicial to the kingdom,' he returned to Newcastle and laid the whole matter before the Mayor. After some consultation with two of the aldermen, it was agreed that Peaker should ride over to Hartlepool, deliver the letter, and return to the Mayor with a report of his mission. Upon his arrival at Durham, Peaker heard that

Colonel Douglas was attending some horse races near Newcastle. He turned back, reported himself to the Mayor, and returned to his duties at the court. In his absence, however, the Mayor had given Leven a hint as to what was taking place, and Leven, on January 1st, spoke to Murray on the matter. Murray at once concluded that Peaker had given information, and accused him of having done so. Nevertheless, 'Master Toby,' as he is called in one news-sheet, was sent, on January 2nd, with a further message to the Dutch captain. Peaker's courage, however, had by now evaporated. Leaving Murray, he obtained a pass from the Mayor, and rode post-haste to York to lay his information before Skippon.

It was not until January 1st, according to Peaker, that Leven was informed of Charles' projected escape. But the news reached London on January 2nd, and as early as December 26th, measures had been taken to secure the King. The Scottish Life-guards had been detailed for sentry duty, and on the 31st four officers from each of the Scottish regiments had been called in to form an additional guard. Even at his games of golf Charles was carefully guarded. In his own apartments at the Court guards were set, and he suffered considerable annoyance from their constant smoking. 'I must tell thee,' he writes to the Queen on January 2nd, 'that now I am declared what I have really beene ever since I came to this army, which is a prisoner, for the go. told me 4 days since that he was commanded to secure me, lest I should make an escape.'

Throughout January the interest of both kingdoms was centred upon Newcastle, the arrival of the various Commissioners, and of the money which was to take both the town and the King out of the hands of the Scots. The money had been sent in thirty-six carts, under convoy from London on December 16th, and had been met at Northampton by Skippon, who proceeded northwards with it. On January 3rd, after a difficult and toilsome journey over bad roads, the money and its escort reached York, and on the following morning the arduous

duty of counting the money commenced. By January 8th no more than £50,000 had been told and the business was expected to require a week's further work.

Meanwhile, on January 7th, the Commissioners from Scotland arrived in Newcastle. They came both to superintend the withdrawal of their army and also to make a further appeal to Charles. Loudon once more urged the King to accept the Covenant, and pointed out that a refusal would leave them no alternative but to hand him over to the English Commissioners upon their arrival. He refused to give a definite answer, but appeared still to contemplate leaving the country altogether rather than to pass into the power of his English subjects. On the 14th, the Commissioners had a further interview with him. He enquired whether he could be regarded as his own master, and if so, what treatment he was likely to receive if he elected to follow their army to Scotland. The Commissioners, however, refused to give him an answer. On the 16th, the English Parliament was informed that the Scotch Commissioners at Newcastle were empowered to concur with the Commissioners from England, upon their arrival, in such steps as should appear necessary for 'the just satisfaction of both kingdoms.' Charles received the news as he was playing chess, but continued the game without showing any emotion. On Sunday, the 17th, a sermon was preached at court by Samuel Kem, at that time attached to Captain Batten's ship, the *Leopard*, on guard duty in the Tyne. The sermon, an elaborate plea for peace, gave Charles considerable satisfaction. He held some conversation with Kem regarding it, and desired to hear him the following Sunday, a hope in which he was disappointed.

On the Saturday following Kem's sermon, the Commissioners from the English Parliament reached Newcastle, accompanied by their chaplains, Mr. Marshall and Mr. Caroll, and by nine gentlemen appointed to attend Charles on his journey to Holdenby. The King was at golf upon their arrival, and Leven was thereby prevented from joining the Mayor and

Scotch Commissioners in the public reception accorded them. On the following day, Sunday the 24th, special sermons were preached at court by an Englishman and a Scotchman, and Charles was as much pleased by the former as he was offended by the latter. Marshall and Caroll, neither of whom Charles could tolerate, preached to the Commissioners. On Tuesday the 26th, he gave audience to the Commissioners. They informed him, shortly, that they were sent to conduct him to Holdenby. Charles replied, that though their message was brief, it yet demanded some consideration, and he would give them an answer in a day or two. He congratulated the Earl of Pembroke on the vigour which enabled him to undertake so arduous a journey, and enquired as to the condition of the roads over which they invited him to travel. To his request that he might be allowed the services of his own chaplains at Holdenby, the Commissioners could give no encouragement.

Already the first £100,000, which had been paid over to the Scots at Topcliffe on the 21st, had reached Newcastle. The remaining £100,000 set out from Northallerton on the 27th, and was expected to arrive by the end of the week. In accordance with the arrangements which had been made, the Scots had withdrawn from their garrisons on the south of the Tyne, while in Newcastle a force of 500 only remained, ready to march out as soon as Skippon entered Gateshead. On the 28th, they established their headquarters at Morpeth, and everything was ready for their final evacuation of the country. On that date Charles made a further endeavour to sound the Scotch Commissioners. He asked them if it was actually their intention to deliver him up? They replied that the course he had adopted left them no alternative. Charles at once sent for the English Commissioners and expressed his willingness to set out with them for Holdenby on the following Wednesday.

On Saturday the 30th, Skippon entered Newcastle and the

Scots marched out of it. Leven had given orders to his troops to treat Skippon and the English "as becometh brethren," so that there might be "a friendly and brotherly parting." On February the 3rd, the day of Charles's departure, the second £100,000 was paid over, and so terminated an occupation of English territory which had lasted since 1644.

For Charles's journey to Holdenby preparation had been made. A grant of £3,000 had been made in Parliament to meet its expense, and carriages and teams of horses had been requisitioned to carry the King's baggage. On February 1st he bade farewell to such of his attendants as were not to accompany him to Holdenby. On the morning of the 3rd, attended by the Commissioners, by the nine gentlemen in attendance upon him, and guarded by a troop of 900 horse, Charles left Newcastle, and by easy marches reached Holdenby on February the 16th.

PROCEEDINGS

OF THE

University of Durham Philosophical Society

(Abstracted from the Minutes).

November 4th, 1897.

(At the College of Science, Professor Lebour in the Chair.)

The following candidates were elected members of the Society :—

Rev. Mark Fletcher, M.A.
W. D. Arnison, M.D.
J. G. Dixon.
Roger Dodds.
G. G. Turner.

The following officers were elected for the session 1897-8 :—

President:
The Very Rev. The Warden.

Vice-Presidents:
Rev. H. P. Gurney, M.A., D.C.L.
F. B. Jevons, M.A., D.Litt.
J. T. Merz, Ph.D., D.C.L.
Professor G. H. Philipson, M.A., M.D., D.C.L.
Rev. A. Plummer, M.A., D.D.
Rev. H. B. Tristram, M.A., D.D., F.R.S.

Committee:
A. W. Ashton, B.Sc.
Robert A. Bolam, M.D.
W. Campbell, A.Sc.
Professor R. Howden, M.A.. M B.
Professor G. A. Lebour, M.A.
Professor R. A. Sampson, M.A.

Hon. Secretaries:
F. C. Garrett, M.Sc. (*Coll. Sc.*)
G. G. Turner (*Coll. Med.*)

Professor Louis and Mr. H. F. Stockdale were elected Auditors.

Mr. F. B. Watson read a note on 'Wireless Telegraphy,' exhibited simple forms of the two instruments used for this purpose, and gave some experimental illustrations, the exciter being in another room, and the waves having to pass through two thick walls before they could reach the receiving instrument.

Professor Lebour exhibited a series of geological models, designed in 1841 by the late Mr. Thomas Sopwith. These models represent in the clearest manner the relations of outcrops to contour lines, the effects of faulting, and those of the intersection of veins. All are made to scale from actual cases observed by Mr. Sopwith in the mining district of Allenheads and Alston.

Mr. Garrett referred to the advantage of printing *Transactions*, and after some discussion, it was resolved 'that if practicable it is desirable that the *Transactions* be printed.' The Committee was instructed to report on the matter.

December 2nd, 1897.

(At the College of Science, Mr. W. Campbell in the Chair.)

On the recommendation of the Committee, it was resolved to print the *Transactions*, and also that the manuscript of any paper which the author desires to publish be sent to the Secretaries, and that it be the duty of the Committee to decide whether these papers be printed in full or in abstract. It was also resolved that a list of members be published in the *Transactions*, the original members being distinguished by an asterisk.

Mr. David Tyzack was elected a member of the Society.

Mr. J. Cooper read a note on 'The Combination of Copper and Sulphur,' and illustrated it experimentally.

Mr. Meek read a paper on, and exhibited a collection of, *Graptolites* from Dobb's Lynn, and another made by Mr. Campbell at Swindale Beck.

Mr. Shaw exhibited some specimens of gold telluride.

February 3rd, 1898.

(At the College of Science, Professor Lebour in the Chair.)

Professor George R. Murray read a paper on 'Ductless Glands and their Secretions,' and exhibited a series of diagrams and photographs.

Professor Lebour referred to the connection between goitre and geology, and spoke of an investigation with which he had been associated, which had for its object, the discovery of the relationship between endemic goitre and the rocks of the affected districts.

Professor Lebour exhibited a very perfect specimen of a stone 'celt' which had been discovered near Boldon by Mr. Sharpe.

March 3rd, 1898.

(At the College of Science, Dr. Merz (Vice-President) in the Chair.)

Principal Gurney read 'Some Notes on the Geology of Finland.'

Mr. L. L. Belinfante gave an account of 'A Journey in Russia during the Summer and Autumn of 1897.'

May 5th, 1898.

(At University College, the President in the Chair.)

The following candidates were elected members of the Society :—

F. O. Solomon. | H. Ward, M.A.
Robert Welford, M.A.

Mr. Terry read a paper on 'Charles I. in Newcastle.'

The President exhibited a miniature of Cromwell which had descended in his family, and in which he is represented with long hair.

LIST OF MEMBERS OF THE SOCIETY.

* *Denotes an original member.*

*Allhusen, E. L., B.Sc.
*Armour, A. L.
*Armstrong, H. E., M.D.
Arnison, W. D., M.D.
*Ashton, A. W., B.Sc.
*Atherton, W. H., B.Sc.
*Baker, T., A.Sc.
*Beanlands, A., M.A.
*Bedson, Professor P. P., M.A., D.Sc.
*Bolam, R. A., M.D.
*Brady, Professor G. S., M.D., LL.D., D.Sc., F.R.S.
*Brown, G. M.
Bryant, Miss A. F.
*Bullerwell, J. W., B.Sc.
*Burkhardt, G. H., B.Sc.
*Cairns, Mrs. B.Sc.
*Campbell, William, B.Sc.
*Caunt, G. W., B.A.
*Chaplin, G. P., B.Sc.
*Cooper, J., B.Sc.
*Cross, H. E., A.Sc.
*Dale, Sir David, Bart., D.C.L., D.L.
Dixon, J. G.
Dodds, R., A.Sc.
Dunn, J. T., D.Sc.
Ellershaw, Rev. H., M.A.
Elsdon, E. J., A.Sc.
Fletcher, Rev. Mark, M.A.
*Fowler, Rev. J. T., M.A., D.C.L.
*Garrett, F. C., M.Sc. (*Secretary*).
*Gravell, John.
*Gray, W. R. H., M.A.
*Gurney, Rev. Principal H. P., M.A., D.C.L. (*Vice-President*).
*Hacking, Thomas, B.Sc.
*Hallaway, R. R., B.Sc.
Hardie, T.
*Havelock, T. H., B.Sc.
*Heawood, P. J., M.A.
Hilton, W. K., M.A.
*Houldey, W. B.
*Howden, Professor R., M.A., M.B.
*Jessop, Professor C. M., M.A.
*Jevons, F.B., M.A., D.Litt. (*Vice-President*).
*Kitchin, The Very Rev. Dean, M.A., D.D. (*President*).
*Knight, R.
Laws, A. R.
*Lebour, Professor G. A., M.A., M.Sc.
*Linton, A., B.Sc.
*Louis, Professor Henry, M.A., A.R.S.M.
Lovibond, J. L., B.A.
*McConnell, W., Jun., A.Sc.
Male, F. J., B.Sc.
*Marston, Rev. H. J. B., M.A.
*Meek, A., M.Sc.
*Mellanby, A. L., B.Sc.
Merivale, J. H., M.A.
*Merz, J. T., Ph.D., D.C.L. (*Vice-President*).
*Morris, Alan C.
Mundella, V. A., B.A., B.Sc.
*Murray, Professor George, M.A., M.D.
Oliver, Professor Thomas, M.A., M.D.
*Patterson, R. J., M.Sc.
*Pearce, Rev. R. J., M.A., D. D.

*PERCIVAL, A. L., M.D.
*PHILIPSON, PROFESSOR G. H., M.A., M.D., D.C.L. (*Vice-President*).
PHILIPSON, W., A.Sc.
*PLUMMER, REV. A., M.A., D.D. (*Vice-President*).
*POTTER, PROFESSOR M. C., M.A.
PROCTOR, A. H.
*REDMAYNE, R. R., M.A.
*RICHARDS, W., A.Sc.
ROLLIN, C., B.Sc.
*ROSS, W. D., A.Sc.
SALKELD, C., B.A., M.B.
*SAMPSON, PROFESSOR R. A., M.A.
*SHAW, SAVILLE, M.Sc.
*SHORTT, REV. J. R., M.A.
*SMITH, R. GREIG, M.Sc.
SOLOMON, F. O.
*SOMERVILLE, PROFESSOR WILLIAM, M.A., D.Œc., D.Sc.
*STOBBS, J. T., A.Sc.
*STOCKDALE, H. F.
*STROUD, PROFESSOR HENRY, M.A., D.Sc.
*TERRY, C. S., M.A.
*TODD, J. J., A.Sc.
*TRISTRAM, REV. CANON, D.D., F.R.S. (*Vice-President*).
TURNER, G. G. (*Secretary*), M.B.
*TURNBULL, W., M.B.
TYZACK, DAVID.
*URWICK, W. E., M.A.
*WADE, THOMAS.
WALKER, REV. DAWSON, M.A.
WARD, H., M.A.
WATSON, STANLEY.
*WATSON, F. B., M.A., B.Sc.
*WEIGHTON, PROFESSOR R. L., M.A.
WELFORD, R., M.A.
WOOLACOTT, DAVID, M.Sc.
*WRIGHT, PROFESSOR MARK R., M.A.
*YOUNG, W. H.

UNIVERSITY OF DURHAM PHILOSOPHICAL SOCIETY.

Income and Expenditure Account. Session 1897-98.

INCOME.	£	s.	d.	EXPENDITURE.	£	s.	d.
To Balance from Session 1896-7	13	12	5	By Printing and Stationery	1	8	6
,, Subscriptions (81 at 5s.)	20	5	0	,, Postages	1	6	0
,, Sale of *Transactions* ...	0	2	0	,, Clerical Assistance ...	0	16	0
				,, Expenses of holding Meetings	2	10	0
				,, Printing *Transactions*	12	10	0
				,, Balance in Treasurer's hands...	15	18	11
	£33	19	5		£33	19	5

Examined and found correct.

HENRY LOUIS,
H. F. STOCKDALE, } *Auditors.*

24*th October*, 1898.

PROCEEDINGS

OF THE

UNIVERSITY OF DURHAM

PHILOSOPHICAL SOCIETY.

VOL. I., PART 3.—1898-9.

NEWCASTLE-UPON-TYNE:
ANDREW REID & COMPANY, LIMITED, PRINTERS AND PUBLISHERS.
1899.

OFFICERS OF THE SOCIETY.

SESSION 1898-9.

President:

THE VERY REV. THE WARDEN.

Vice-Presidents:

REV. H. P. GURNEY, M.A., D.C.L.
J. T. MERZ, Ph.D., D.C.L.
PROFESSOR G. H. PHILIPSON, M.A., M.D., D.C.L.
F. B. JEVONS, M.A., D.Litt.
REV. A. PLUMMER, M.A., D.D.
REV. CANON TRISTRAM, M.A., D.D., F.R.S.

Committee:

T. BAKER, A.Sc.
R. A. BOLAM, M.D.
PROFESSOR G. S. BRADY, M.D., LL.D., D.Sc., F.R.S.
PROFESSOR R. HOWDEN, M.A., M.B.
PROFESSOR R. A. SAMPSON, M.A.
MISS K. A. SMITH, A.Sc.

Chairman of the Chemical and Physical Section: F. C. GARRETT, M.Sc.
Secretary: T. BAKER, A.Sc.

Chairman of the Biological Section: PROFESSOR M. C. POTTER, M.A.
Secretary: A. HOWSON.

Honorary Secretaries:

W. CAMPBELL, B.Sc.
F. C. GARRETT, M.Sc.

UNIVERSITY OF DURHAM PHILOSOPHICAL SOCIETY.

ON THE NATURE AND ORIGIN OF FRESH-WATER FAUNAS.

By G. S. BRADY, M.D., LL.D., D.Sc., F.R.S.

[Read November 3rd, 1898.]

It has been cynically said that the use of language is to conceal one's thoughts, and if it be not quite true that the use of a title to a paper is to mystify one's audience, it may at least be admitted that it is often by no means an easy task to invent one which shall be accurate without circumlocution. I do not pretend to have succeeded in the quest of a title: let me a little amplify that which I have chosen. I want to bring before you this evening some considerations respecting the first appearances of life in the sea and in fresh-water, respecting the present character of the fresh-water fauna, and lastly respecting some of the causes which have probably been concerned in its development.

Accepting, as almost all naturalists now do, the general idea of evolution, we must believe that all organic life had its beginning in the most simple form, and that those complexities and differentiations of structure which we now see everywhere around us have been gradually acquired, partly perhaps as the result of competition in the race for life, partly in response to the action of external conditions, and still more probably from causes of which we as yet know little or nothing. And if it has become evident that 'natural

selection' has not been the all-sufficient cause of specific development—as some of the followers of Darwin have believed—let us remember that that great idea still holds the field, and that even if it passed out of sight to-morrow, the observations upon which it was built, and the clear enunciation of it as a working hypothesis, gave a new impetus, a new life to the study of Natural Science, with which the name of its illustrious author will ever be indissolubly associated. The present generation has, of course, been nurtured from its birth on the Darwinian idea, and it is perhaps only those amongst us who remember the dry bones of the pre-Darwinian era who are able adequately to appreciate the rejuvenescence which followed.

The simplest conceivable form of life—seeing that life cannot exist apart from protoplasm—would be a speck of undifferentiated protoplasmic material. But we know of no living organism quite so simple as this. There is usually an external envelope or cell-wall, and an inner vital point or nucleus, without which it appears that reproduction is impossible. There are doubtless cases in which this nucleus has not been observed, but such evidence is at the best inconclusive, and the chances are that sooner or later all actively reproducing cells will be found to be nucleated. A spherical, nucleated cell, microscopic in size, we may take to be the most primitive independent organism. Many such creatures are known to us, all of them inhabitants either of water or of very moist media. An aquatic environment would seem necessary to preserve the moisture of the cell and to allow of gaseous interchange between it and the surrounding medium. Vital processes resulting in waste and demanding recuperative supplies from outside, need of course such interchange. But there is a very definite limit to the growth of such a cell. Herbert Spencer has pointed out that inasmuch as the periphery of a sphere increases only in the ratio of the square of its diameter, the contents increase as the cube of the

diameter, and seeing that the cell-contents are its vitally active portion, there must come a time when increase of bulk outruns the capacity of the periphery for the necessary nutritive and excretory processes. In this predicament the required increase of surface might be obtained by the indenting or invagination of the cell wall, which would probably be the first step in the upward process, and would be followed by still further amplifications of secreting and excreting surfaces, resulting ultimately in those elaborations of structure which are characteristic of the higher animals. And as a matter of fact this invagination is the first step in the development of the egg, constituting the structure called a Gastrula—a stage characteristic of the higher animals,—of the Metazoa as opposed to the Protozoa.

That life originated in the sea is an article of the orthodox zoological faith. In the shallow littoral region, where warmth, shelter, and the action of the tides would greatly favour both animal and vegetable growth, we may suppose that the first tentative efforts at life occurred. From this zone, under the influence of that struggle for existence which would compel migration to new and unexploited regions, we may believe that the depths of the sea on the one hand, and on the other rivers, lakes, and the dry land would eventually become populated.

How very real this struggle is,—how every nook and chink and cranny which may afford a means of livelihood is quickly seized upon, every observer could easily illustrate from his own experience. Let me quote one or two familiar instances. Dr. Fritz Müller found in little reservoirs of water contained at the bases of the leaves of a Bromelia, in South America, numbers of a small bivalved Crustacean belonging to a group of Entomostraca commonly found in lakes and ponds: this species *(Elpidium Bromeliorum)* having found the little Bromelian tarus untenanted, and offering a pleasant retreat 'far from the madding crowd's ignoble strife,' house rent

free, water laid on, and no rates nor taxes, wisely accepted the tempting offer, and has not as yet been ejected by any other aspiring tenant, though we may be sure that there are plenty such prowling around and ready to take advantage of any little illness or misfortune befalling the present proprietor. Very recently I had sent to me specimens of an Entomostracan belonging to a nearly related group—Copepoda—which had been found in a similar position on a Bromeliaceous plant in the gardens of the Royal Botanical Society: this is, I think, an undescribed form. But, on the other hand, some plants, resenting this kind of intrusion, have developed traps and digestive juices which make short work of the intruders. The bladders of *Utricularia* and the pitchers of *Sarracenia* are cases in point: insects entering these precincts are in the first place prevented by mechanical contrivances from getting out, and are then digested by the action of an acid peptic juice.

A general comparison of the present fresh-water fauna with that of the sea shows that one very important group of marine animals—*(Echinoderma)*—is altogether absent, and that another large group *(Cœlentera)* is only very feebly represented. Only two classes have a preponderance in fresh water—the Amphibians (frogs, newts, and the like), which are altogether terrestrial or fresh-water in habitat, and Insects, which although not largely represented in fresh-water, are even more scarce in the sea. But this part of the subject must be considered more closely. To begin with the most lowly organized group: amongst Protozoa, the two large classes of Radiolaria and Foraminifera are almost without exception exclusively marine, and seeing that a very closely allied group, Amœbea, has established itself firmly in fresh-water, where it is almost ubiquitous, it is not easy to understand why its near relatives have failed to do so. Foraminifera of some kinds are abundant in the estuaries of tidal rivers, and may be found even in the pools of marshes where the water is

only very slightly saline, but further than this they do not seem able to penetrate. And in such localities they are invariably depauperated, the tests becoming thin and deficient in lime. The salt marshes of our own district supply many interesting illustrations. It is possible that absence, or diminished quantity, of lime in the water may be a chief cause of the absence of Foraminifera, just as the sub-acidity and 'softness' of the water of peat-mosses seems to render it unfit to support Microzoa with calcareous shells, while those with merely chitinous valves are often abundant.

Sponges are almost wholly marine in their distribution: they comprise about forty families, only one of which belongs to fresh-water. Of the Cœlentera (jelly-fishes, zoophytes, &c.) only three families out of a total of about seventy are represented in fresh-water, these being the well-known *Hydra* and *Cordylophora*, and two small Medusæ—*Limnocodium*, found in the Victoria-regia tank of the Botanical Gardens, Regent's Park, and another, *Limnocnida*, in Lake Tanganyika. The heterogeneous group Vermes includes many fresh-water forms, but the larger and more important section Arthropoda is very imperfectly represented. This division includes Crustaceans, Spiders, Centipedes and Insects, the first of these classes occurring abundantly in the sea, but, if we except the microscopic Entomostraca, not making much show in fresh-water: in our own country the only fresh-water delegate from the larger Crustaceans is the common Crayfish; the Spider class shows up fairly well in fresh-water, but is absent from the sea, if we except the Mites and Pycnogous. Centipedes are wholly terrestrial and insects almost wholly so, only very few marine genera being known: in fresh-water, however, they are more abundant, especially during the larval condition. The Mollusca (shell fishes of the whelk and oyster type) are chiefly marine, though some families are largely represented in fresh-water. Of aquatic vertebrates, fishes are of course the most important; here, of 137 families, 35 are found in fresh-water,

but many of these very sparingly. The Amphibians have already been mentioned as entirely absent from the sea.

Speaking generally, then, it appears that very few animals have been able to establish themselves in fresh-water, and those which have succeeded have been enabled to do so by very various devices and modifications of structure. It may be interesting to refer briefly to a few of these. Professor Sollas, in a very interesting paper published fourteen years ago, suggested that amongst the principal causes that have interfered with the spread of animals to fresh-water may be reckoned, *firstly*, the prevalence amongst marine animals of larval forms so feeble as to be unable to withstand river-currents; *secondly*, prejudicial fluctuations of temperature in fresh-water; *thirdly*, disturbing physical causes, such as floods, droughts, upheavals and depressions of surface, etc. The consideration, however, which most naturally suggests itself is the non-salinity of fresh-water, which might be expected to exert a powerfully deterrent influence. But this would probably not have been in itself insuperable; many fishes, such as the salmon and lamprey, live alternately in fresh and salt water, and M. Beudant found in the course of experiments on the marine mollusca that by the very gradual addition of fresh-water a large proportion of the animals could be thoroughly acclimatised. In these experiments thirty-seven per cent. died; but as a check-experiment, out of those kept in captivity in *salt* water thirty-four per cent. died, so that want of salt cannot have been to any great extent the cause of mortality. Bearing on this question there is the often-quoted case of the Brine Shrimp, *Artemia salina*, a graceful little Crustacean which used to inhabit the salt-pans of Lymington and other places in England, and may still be found in brackish water abroad. This creature lives in water containing about four per cent. of salt, but in much stronger brine, containing as much as twenty-five per cent., there occurs another slightly different form *A. Müllhausenii*, and it seems

to have been proved that these two forms are transmutable, their special characters depending entirely upon the degree of salinity of the water. It has even been stated that the transformation may be carried still further, so as to produce an entirely distinct genus, *Branchipus*, but this statement rested undoubtedly on erroneous observation. It is scarcely likely then, sufficient time being given, that the absence of salt would of itself interpose an insuperable barrier to the acclimatisation of marine life in fresh water.

A very interesting illustration of the effects of varying salinity is found in the shells of the Aral Sea cockle. The 'terraces' round this inland sea contain many cockle-shells and have been carefully studied on the spot by Mr. Bateson, to whom I am indebted for the series of specimens shown to you this evening. The Aral Sea has for long been gradually contracting its area owing to loss of water by evaporation, its former boundaries being indicated by a series of terraces extending inland—in some places for many miles—to a great distance from the present basin. These terraces contain shells of the common cockle (*Cardiuno edule*) which, when compared with those now living in the lake, show gradually progressive alterations of character from the earliest and least saline condition of the water down to the present time – alterations affecting the size, thickness, sculpture and colour of the shells. Variations of similar kind are found in the cockles of our own country—depending, doubtless, on varying environment.

A remarkable instance of recent migration is that of the Hydroid Zoophyte, *Cordylophora*, which, first found by Professor Allman in 1854, in the Docks of the Grand Canal, Dublin, is apparently extending its area of distribution, having been more recently found in the Seine and in the Norfolk Broads, while in the water-pipes of Hamburgh it has become so rampant as to partially block them. Assuming this extension of area to be real, and not merely a result of closer observation, the question arises, How has dispersal

been effected? There are special difficulties in the case of *Cordylophora* to which I will return later on, but as bearing on this part of the subject, Mr. Clement Reid's interesting study on the distribution of plants and molluscs in isolated ponds may be briefly noticed. The 'dew-ponds' of the South Downs are wholly artificial - made for the sake of the cattle in an almost waterless district. They are fed solely by rain and by percolation of water through the soil, are mostly far from roads and houses, and at elevations of several hundred feet above sea level. Yet when once made they very soon begin to be populated, and in time come to support a tolerably extensive fauna and flora. Water carriage of germs is of course here out of the question. Ova of minute animals, such as rotifers and entomostraca, may sometimes be desiccated, blown about as dust, and so find access to the ponds in a living condition; but seeds or ova of plants, and larger animals like mollusca, are more likely to be carried from place to place on the feet of birds, entangled often amongst shreds of algæ or other aquatic plants. I have myself made a rather cursory examination of some of these Sussex ponds, with a view chiefly to their crustacean contents, but have unhappily mislaid my notes. The general result, however, was that they contained a somewhat restricted entomostracan fauna similar in kind to that of the surrounding district, and I am disposed to think that transference by birds, even of these microzoa, is a more probable means of dispersal than the agency of winds and dust.

A point specially emphasised by Professor Sollas is the very feeble swimming capacity of the larva in many marine animals, an observation, however, in which Sollas seems to have been to some extent anticipated, as he himself states, by Semper. This, one can scarcely doubt, must have had a distinct influence on distribution. A vast number of marine invertebrates leave the egg as 'ciliated planules,' minute microscopic structures which swim freely, but only by means of very

delicate cilia, and are thus entirely at the mercy of currents and external influences : these organisms could, of course, make no headway against even a feeble river current, and would never be able to establish themselves in such positions. We do not, therefore, find larval forms of this kind amongst animals inhabiting running water, though they do occur not infrequently amongst lacustrine species. It would seem, indeed, that to hold their own against river currents animals must be hatched in a condition suitable for immediate attachment to a fixed base or with the mature muscular activity of the parent. The two Medusæ already referred to are interesting examples of a feeble larval condition, and, seeing that they belong to a typically marine group, it is not easy to account for their presence in fresh-water lakes : it may safely be said that they cannot have got there by way of river channels, but they must nevertheless have originated from some marine medusoid form. In the case of the African species—*Limnocnida*—a possible explanation may be this : it is believed that during the later Eocene period a great sea covered the northern part of that continent, stretching from the Mediterranean to the Bay of Bengal. On the retreat of this sea owing to elevation of the land, the great lake basins—Tanganyika and others—were left behind, and in them we have the remains of a modified marine fauna. (Not only Medusæ, but fishes of types hitherto looked upon as purely marine occur there.) The necessary adaptations of the adult animal once effected, the ciliated embryos would of course be subject to no more risk than in the waters of the sea. That such relics of old marine populations do exist is shown by the presence in some of the lakes of Britain and Scandinavia of certain Crustaceans which appear to have been left behind after the retreat of the sea from those areas, and to similar causes may perhaps be attributed a not insignificant portion of the present fresh-water fauna.

Among the lower fresh-water animals sponges constitute a very restricted but interesting group, which seems to have

adapted itself to the new conditions in a remarkable way. The ordinary method of reproduction in sponges is by means of ova, which hatch out as free-swimming ciliated larvæ—soon, however, losing their cilia and becoming attached and stationary. The fresh-water sponges, however, are able to live in running water, where ciliated larvæ would be impossible. The difficulty has been solved by the production towards the close of summer of numerous hard-coated, spherical 'gemmules,' which remain embedded in the substance of the parent sponge, are set free by the decay of the protective tissue, and develop directly into new sponges. This process seems to be chiefly a provision against the effects of extreme winter temperatures, which might, and probably often do destroy the parent sponge, but are successfully resisted by the gemmules. In marine sponges, which are not exposed to the same vicissitudes of temperature, reproduction by gemmules, except in the case of a few species, is unknown.

One remarkable modification which may have helped largely in the establishment of a fresh-water fauna is the comparatively large size of their ova. The great number and minute size of the eggs in most marine fishes are familiar facts. A mackerel, for instance, is said to produce from 80,000 to 90,000 eggs, a cod 5 millions, a conger-eel 15 millions, while those of the salmon, spawning in fresh-water, are very much larger and of course fewer. The smaller an egg the less nutritive yolk it contains, consequently the embryo must hatch out earlier and in a comparatively small, immature, and helpless condition; the vast majority of the fry, therefore, fall a prey to voracious neighbours, likely enough eaten by their own parents. But the young salmon, hatched from a larger egg, is stronger, more fully developed, and better able to hold its own against an adverse environment.

Again, the river crayfish, though a smaller animal than the common lobster, has much larger eggs, and these are hatched in a form very like the parent, but specially provided against

the stress of currents by temporary hooks, which enable it to grasp and hold on to the maternal feet. The larval lobster, on the contrary, is poorly developed and minute, and has to pass through several free swimming phases before attaining the adult form; it would not have a chance for life in a running stream. But amongst Crustacea, the only fresh-water group which has been thoroughly successful is that of the Entomostraca, which reproduce in very various fashion: some are viviparous, some produce ova which are always relatively large, but which may undergo extensive transformation, some have taken to parasitic life and become very much altered in consequence, while others have adopted a supplementary system of eggs meant to resist vicissitudes of climate. None of these, though strong swimmers even in their early stages, would have been able to migrate against a river current, though well able to maintain themselves in slowly moving water. But we have seen how soon they may make their appearance in new and isolated waters like the dew ponds of the South Downs, brought thither by winds or by birds, and in this way it is easy to account for their existence in lakes and ponds.

An interesting fact connected not perhaps with the origin but certainly with the maintenance of the fresh-water fauna, and especially noticeable amongst Entomostraca, is the extreme transparency and often the extreme tenuity and slenderness of limb in deep-water forms. These characters, however, are shared equally by marine and fresh-water species. The transparency and absence of colour would seem to be protections against the voracity of larger animals, and may perhaps, as some of them are known to be exceedingly voracious, assist them in their own depredations among smaller ones. The abnormal length of limb probably gives agility in swimming, and could never have been developed except in absolutely still water: amongst the vegetation of the shallows and the troubled waters of the surface such structures would speedily be maimed or entangled. Thus it is only amongst littoral forms, living

amongst weeds and surroundings of varied colour that we find conspicuous coloration, and correlated with this a marked compactness of external form.

Cordylophora, already referred to, has, like most other hydroids, a free swimming larva; hence the difficulty in accounting for its rapid extension of area in fresh-water. Further observation is needed respecting it, but it has been pointed out that in its migratory movements it is largely associated with *Dreissena polymorpha*, a mussel-like mollusc which is spreading in a similar fashion. The only other fresh-water cœlenterate, the common *Hydra*, differs altogether in its reproductive methods from *Cordylophora* and the Medusæ: it has thoroughly accommodated itself to external conditions by producing very large ova which hatch out at once almost in the adult form.

On a Bacterial Disease—White-Rot—of the Turnip.

By Professor M. C. Potter, M.A., F.L.S.

[Abstract of a paper read December 8th, 1898.]

The author has found in the early autumn numerous turnips whose roots, when fully grown, have become completely rotten. The rotten portion presents a white, glazy appearance, and the tissues are reduced to a soft, pulpy condition ; the cell-walls are much swollen, faintly stratified, and separate from each other along the middle lamella. The decaying mass is infested with bacteria, but the most careful microscopic search has failed to detect any fungoid hyphæ, and no fungi have appeared in the experimental cultures.

The rottenness can be readily introduced into a sound root by inoculation at a wounded surface ; from this point the decay spreads rapidly through the root, the leaves gradually turn yellow, and in about fourteen days the entire plant has succumbed.

Among the bacteria found in the rotten mass one has been isolated, which, when sown from a pure culture on to turnips, under sterile conditions, induces all the characteristic effects of the "white-rot." The liquid expressed from the pulp of one of these cultivations, when passed through a Pasteur-Chamberland filter, yields a clear, light yellow filtrate, which was found to have the same powerful action upon the living cells of the turnip, causing the swelling of the wall and the separation of the cells by the dissolution of the middle lamella. This action was destroyed by boiling. When diluted with four to five

volumes of alcohol, the extract yields a copious flocculent precipitate; the precipitate was dried and digested with a little water, and the solution, after filtration through the Pasteur-Chamberland filter, was also found to have the same effect upon the cell-walls, the action again being destroyed by boiling. The whole appearance of the sections corresponded exactly with those taken from turnips affected by the rot. The bacterium, therefore, secretes a cytase enzyme, which, in healthy living tissues, dissolves the middle lamella and causes the swelling of the cell-wall. The same enzyme is produced when the bacterium is grown in Koch's bouillon.

A diastatic enzyme, and one which converts lignin into cellulose, are also secreted.

The bacterium has a single polar flagellum, and, adopting Migula's classification, the author has ventured to describe it under the name *Pseudomonas destructans.*

General Characteristics.

Short rods about 3μ long by $\cdot 8\mu$ broad, with one polar flagellum.

Rapidly liquefies gelatine, forming circular whitish colonies in petrie capsules.

Agar-agar, whitish glazy growth.

Stab cultures grow along the track of the wire, rapidly forming a funnel.

Aerobic.

Parasitic on turnips, potatoes, carrots, but not beetroot, forming a cytase.

Copious evolution of carbonic acid during the fermentation.

Infection by *P. destructans* appears always to be introduced at a wound, and it is powerless to set up decay unless placed in contact with the parenchymatous cells of the cortex. Having gained an entrance, the organism penetrates the living tissues by means of the intercellular spaces, and by the action of the

enzyme it breaks through the intercellular substance and traverses the middle lamella. Many of the intercellular spaces are crowded with bacteria, and in some sections they are found lying in the track of the middle lamella.

Enzymes, similar in nature to that described above, have been demonstrated by Marshall Ward for *Botrytis* and de Bary for *Sclerotinia*. In many parisitic fungi the hyphæ also travel along the middle lamella in the thickness of the cell-wall. The action of the bacteria then upon living plant tissues is shown to be strictly homologous with that of the parasitic fungi.

P. destructans flourished upon nutritive media, and, even after many cultivations, could readily be inoculated from these on to pieces of living turnip, producing all the effects of the rot in about twelve hours ; cultures, both on nutritive media and on the turnip also rapidly invaded the tissue of the potato. Whether, therefore, it has any existence in the saproptytic form or not, it has evidently become strongly established as a parasite attacking the turnip, and probably is not confined to the turnip alone.

Bacterial disease of turnips is much more common than is generally recognised, and the one now described is often very destructive to the crops. Many other organisms, either singly or in combination, play a very important part in the destruction of living turnips and swedes. In addition to Bacteria and *Plasmodiophora brassicæ*, the author has found the Cruciferous crops to be attacked by *Fusarium*, *Botrytis*, and also by *Pythium ;* and it is probable that these do not exhaust the list of vegetable parasites for this crop, but further research is necessary before it is possible to separate the various organisms and assign to each its rôle.

On the Post-Embryonal History of Voluntary Muscles in Mammals.

By Alexander Meek, M.Sc.

[Read January 19th, 1899.]

The growth of mammals may be said to start with the division of the ovum, a cell which, though much the same in size and appearance in all the placental mammals, yet possesses that within it which determines not only the species and even the variety of the species, but the individual. Growth during the early part of the first period of existence—the intra-uterine—is rapid, and is characterized by cell division, but from the very beginning it gradually slows down, and the share which cell proliferation takes in framing the organs and tissues naturally also becomes less. Growth still proceeds, however, during the whole period of intra-uterine life—a period which is very constant for the species—and during a succeeding part of the extra-uterine life, and this period of extra-uterine growth is also very constant for the variety and the species. Even when the seemingly static adult condition is reached, cell multiplication still takes place as a normal accompaniment of the life-work of certain of the tissues. The epithelial cells of the epidermis and mucous membranes are shed and replaced at all stages of life. Bone* is capable of considerable modification, even at a late period in the extra-uterine history, long after growth has ceased. During growth it is characterized by deposition and absorption, but even in the adult stage bone may still be absorbed and deposited according as the pressure is increased or decreased, and we may presume like-

* A. M. *The Veterinarian*, 1897, and these *Proceedings*, vol. i., part 1.

wise according to the life-work of the individual. Many of the other varieties of connective tissue preserve in like manner considerable powers of reproduction.

But while this power of adjustment and regeneration is a natural attribute of the simpler tissues and cells, the more specialized nervous and muscular tissues appear to be very different in this respect. The general slowing down in the process of proliferation during intra-uterine life becomes most marked in these structures. We have no knowledge of a multiplication of nerve cells during extra-uterine life, and the process of cell division in the highest mammals seemingly ceases before birth.* We thus apparently start life with all the nerve cells we are to have, but it is very questionable whether we draw the whole of them into use before the end! The power of regeneration, however, is strong in the nerve fibre, which is a process of the nerve cell. This has been often shown by experiment. In the deer a natural replacement, involving bone, periosteum, and skin, as well as vessels and nerve fibres, takes place every year with the shedding and subsequent growth of the antlers.

Voluntary muscular tissue, popularly 'flesh,' consists of long fibres, which are bound together by fine connective tissue into small bundles called fasciculi, and these again are bound together to form the muscle. The number of fibres entering into the fasciculus and the number of fasciculi forming the muscle vary in different muscles; and while, of course, for the same muscle the difference is great when different species are contrasted, it is evident also in comparing individuals of the same species, and even of the same family.

This brings us to the question which the research here briefly described was an attempt to answer. During growth, are the muscle fibres increased in number, or do they simply increase in size? Preliminary experiments were made by

* *Vide* Ziegler's and Hamilton's *Text Books of Pathology* and Donaldson's *Growth of the Brain.*

breaking up the muscle into fibres to see how far a counting of a cross section might be relied upon. Longitudinal sections were compared with transverse sections, and a whole series of transverse sections through the whole of the middle part of the chosen muscle were compared with one another; and it was gratifying to find that the fibres extended far beyond the part selected, and that no essential difference in number and arrangement could be detected in widely separated sections. It was decided, therefore, to carefully isolate in the individuals of known age the middle third of the muscle selected, and, after treating in the usual way, to cut this in each case into transverse sections. A typical section was counted for each stage, and the results tabulated.*

The figures, without exception, showed that while the muscle as a whole certainly increased in size the fibres actually decreased in number. This will be evident from the following statement of the more important results.

(*a*) Young and Adult of the Field Vole (*Arvicola agrestis*).

LEFT TRICEPS.

	No. of Fibres.	Aver. per Square of ·151 mm.	Area of Section. mm.	Aver. Area of Fibre. mm.	Percentage of Fibres.
Young male	10,070	114	1·95	0·0002	100
Adult female	4,613	17	4·96	0·0013	46

LEFT STERNO-MASTOID.

	No. of Fibres.	Aver. per Square of ·151 mm.	Area of Section. mm.	Aver. Area of Fibre. mm.	Percentage of Fibres.
Young male	2,696	—	—	—	—
Adult female	1,128	—	—	—	—

RIGHT STERNO-MASTOID.

	No. of Fibres.	Aver. per Square of ·151 mm.	Area of Section. mm.	Aver. Area of Fibre. mm.	Percentage of Fibres.
Young male	2,596	131	0·4389	0·00017	100
Adult female	2,110	37	1·2426	0·0006	81

* A preliminary communication was published in July, 1898, in the *Anatomischer Anzeiger*, band xiv.; and since the present paper was presented a further note has appeared, also in the *Anatomischer Anzeiger*, band xv., as well as an article in the *Journal of Anatomy and Physiology*, 1899, vol. xxxiii., p. 596, to which the reader may be referred for details.

(*b*) Left Biceps of Cat (*Felis domestica*).

Age.	Sex.	Number of Fibres.	Average per Square of ·0263 mm.	Area of Section. mm.	Area of Fibre. mm.	Percentage of Fibre.
9 days ...	Male ...	83,514 ...	7·0 ...	8·4 ...	0 0001 ...	100
20 days ...	Male ...	64,108 ...	5·5 ...	8·1 ..	0·00013 ...	77
240 days ...	Female ..	37,830 ...	1·15 ...	22·8 ...	0·0006 ...	45
3 years 5 months	Female*	32,039 ...	0·54 ...	41·5 ...	0·0015 ...	38
Adult	Male† ...	22,858 ...	0·34 ...	46·8 ...	0·0021 ...	27

(*c*) The Sheep (*Ovis aries*).—One of the Outer Heads of the Perforans.

Age.	Sex.	Number of Fibres.	Aver. per Square of 0263. mm.	Area of Section. mm.	Area of Fibre. mm.	Proportion of Fibres.
Embryo ...	♀ ...	29,174 ...	10·5 ...	1·916 ...	·00007 ...	42
2-3 weeks ...	♂ (?) ...	69,694 ...	4·0 ..	12·057 ..	·00017 ...	100
6 months (?) ...	♂ (?) ...	31,978 ...	1·0 ...	22·117 ...	·00069 ...	46

Similar results were also obtained in an examination of certain of the muscles of the tame rat. It was found, moreover, that apparently the conditions of extra-uterine life do not always affect the muscles to the same degree on both sides of the body; that variation becomes most marked the older the specimens; that the female possesses a less number of fibres than the male at the same age.

It will be noticed also that the hypertrophy of the fibres which are selected or survive, and the reduction in the number of fibres are expressive of the functional importance of the muscle. Indeed, the reduction in number (aplasia) is intimately related to the hypertrophy of the survivors. This is evident on comparing the above figures.

Thus, while it is a fact that the ovum may develope into an animal which expresses clearly the qualities we call hereditary, the final result is modified by environment. In regard to

* This was the mother of the three preceding and those three belonged to the same litter.

† This specimen was obtained from the Newcastle Dog and Cat Shelter and was guaranteed at least 3 years old. The figures show that it was much more than 3 years old, and its appearance and the teeth had already led to such an impression.

muscle fibre in the placental mammals at least, the growth by proliferation appears to cease sometime before or not long after birth, and then extra-uterine life brings about a competition amongst the fibres resulting in a survival of the fittest. The hypertrophy of certain of the fibres brings about aplasia of the unfit or less fit; and as these changes will be hastened by exercise and retarded by rest, the importance of environment and the conditions of life becomes manifest.

Perhaps, however, these considerations may be best given by quoting the concluding paragraphs from the paper in the *Journal of Anatomy and Physiology*, where details of the investigations may also be found :—

In this connection comes a question of much importance, to which an answer may be attempted. It is, as to the effects which exercise—in the case of the athlete—and rest and feeding—from the point of view of the farmer principally—will have in modifying the changes which have been described.

We have seen that the facts appear to show the conditions described above are intensified the more important the muscle. The survival of the fibre, in fact, will be determined by the measure of the contraction, and that by the nervous impulse reaching it and the power of reconverting it. On the other hand, rest and relatively nutritious food must reduce this tendency to select some fibres at the expense of the others, and therefore make the muscle richer in fibres—if the muscle does not reach so high a plane of development. We hear the farmers say that in feeding the best results are obtained by never letting the animal 'lose its calf-flesh.' That is to say, the animal must, by getting suitable food, be kept steadily growing. Relative starvation will necessarily hasten the tendency to atrophy.

The question of 'feeding,' then, from the farmer's point of view, is not one of making more flesh, but rather by observing the rules which practice has so long recommended, of hindering the reduction in the number of fibres, to which all flesh seems to be to a greater or less extent liable

The development of flesh in the feeding animal and the development of the muscle of the athlete are, in fact, quite opposite in their results.

In the first case, the farmer's aim is to keep as many of the fibres as he can by not exercising too much, and by keeping the blood circulating through the muscles in a rich condition. Nevertheless, the stimulation to the circulation which some degree of exercise gives must not be lost sight of. Covered courts and similar conditions, when confinement is

necessary, which admit of the animals moving about freely, seem therefore, with regular feeding, and other considerations of treatment which need not be entered into here, to be the nearest approach to the ideal state of things.

In the case of the athlete, the exercise hastens the choice and development of suitable elements, and the muscle gains in bulk through the hypertrophy of these. It is well known, moreover, that if the exercise be not kept up, the muscle fibres return to some approach to their old condition, and may even sink beneath it. Thus the conditions which we have been considering as affecting muscle as a whole, are liable to much variation from external influences. The influence of 'internal secretions' is too well known to require here more than mention ; and it is quite clear, moreover, that the pressure of the fibres upon one another, and its results, will be subject to the influence of the relative pressure of neighbouring muscles and other organs.

To sum up. The life-history of muscle seems to me to be determined by (1) inherited qualities, present in the fertilised ovum, the evolution of which is controlled by (2) internal influences—internal secretions (including the effects of 'sex'), the mutual influence of the muscles upon one another, and of the fibres upon one another, and the internal variations amongst the fibres; and by (3) external circumstances—work, food, habit, and, indeed, the ordinary and extraordinary conditions of extra-uterine life. Up to the time of birth, in at any rate the higher mammals, perhaps in all the Eutheria, hyperplasia characterises the growth of muscle ; while after or about birth, hyperplasia ceases, and extra-uterine life brings about a selection of some of the fibres at the expense of their neighbours. In other words, during extra-uterine life, muscle, according to its position, suffers more or less a reduction in the number of its fibres, the degree of which is expressive of its functional importance. Tho surviving elements are at the same time greatly hypertrophied, and the extent to which this takes place is also expressive of the work which the muscle performs, or of which it is capable.

A Case of Congenital Malformation of the Liver in the Sheep.

By Alexander Meek, M.Sc.

[Read March 23rd, 1899.]*

This abnormal liver, when it was added to our collection in December, 1898, through the kindness of a local butcher, was seen to consist of a long tongue-shaped mass depending from the diaphragm, with a gall bladder of the normal size lying altogether free from it. A further analysis showed the condition of things now described.

The tongue-shaped mass which was first seen is the immensely developed caudate process of the spigelian lobe, or, as the butchers call it, the 'thumb' of the liver, while the rest of the liver is more or less atrophied. The left lobe appears to be altogether absent. There is a slight remnant of the quadrate lobe between the cystic and hepatic ducts. This is continuous through the spigelian lobe with the remnant of the right lobe, a remnant representing mainly the attached portion of the spigelian lobe. Even the body of the latter is slightly reduced, and the processus papillaris; but the free or caudate lobe has attained to several times its ordinary size, forming the long tongue-like structure already mentioned The reduced portions of the liver were closely attached to the posterior surface of the diaphragm. Towards the left the spigelian and quadrate lobes thin away to an ill-defined border. The length of the abnormally-developed caudate lobe from the post vena cava to the free end is 9 inches,

* This paper also appeared in *The Veterinarian* of May, 1899. I am indebted to the proprietors of that journal for permission to use the two blocks, Figs. 1 and 2.—A. M.

and the greatest width about 4 inches, the corresponding measurements in the normal condition being about 4 inches and 1½ inches respectively.

Abnormal conditions in the liver are not uncommon; but as the laws of development may often be better studied in their

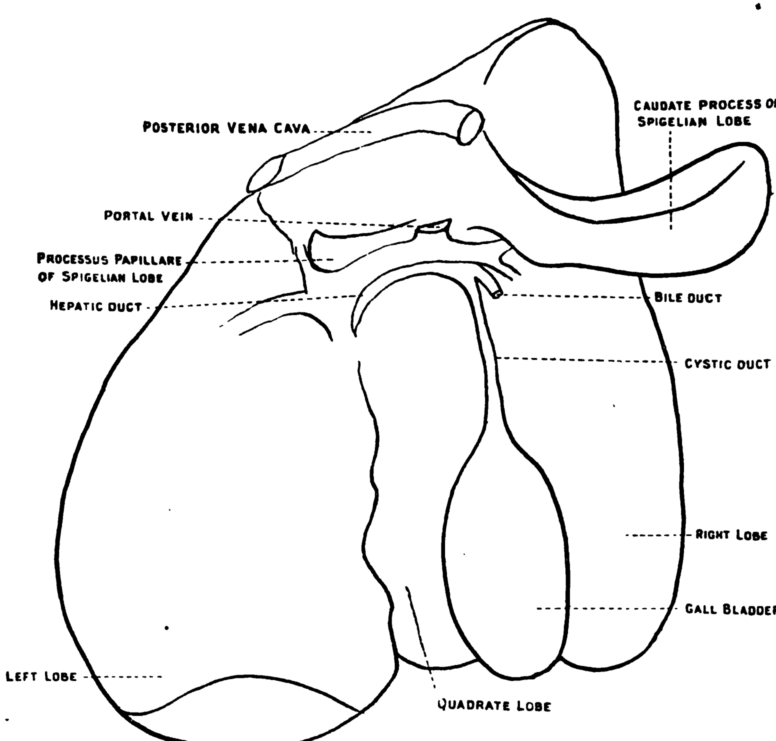


FIG. 1.—NORMAL LIVER OF SHEEP.

breach than in their observance, the recording of such cases is a duty we must not neglect.

There are some, perhaps, who might read in the above described curious example an attempt at reversion to the condition of Petromyzon, but such speculations we are scarcely

yet prepared for, if they ever will be desirable. It seems clear whatever the cause has been, whether internal or external to the structures concerned, that the rudiments of the lobes had got out of their normal state of equilibrium in development. The result

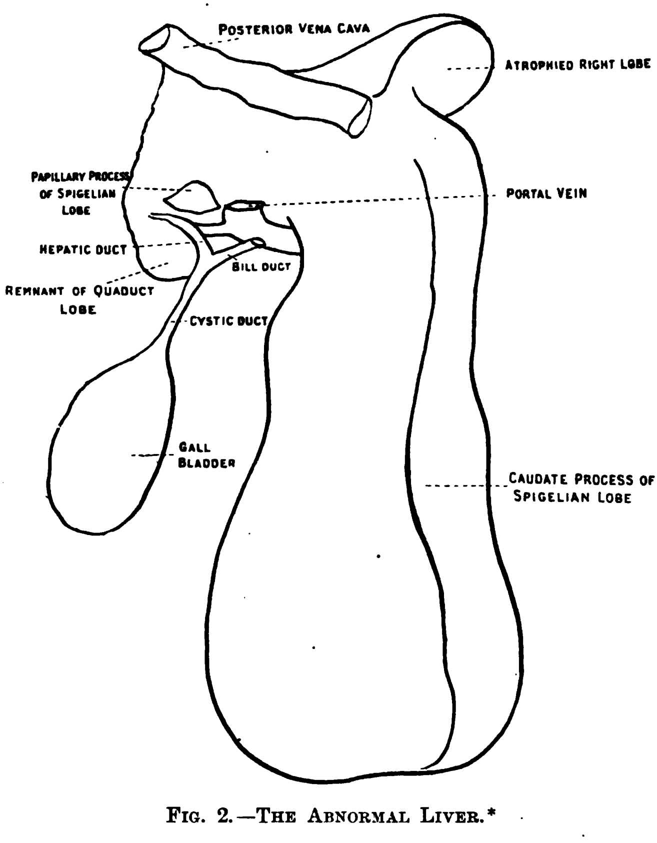


FIG. 2.—THE ABNORMAL LIVER.*

in this case was the giving of a start to the spigelian lobe, whose overgrowth or hypoplasia led to a complete suppression of the left lobe and considerable hypoplasia and, perhaps, aplasia of the right lobe. These terms are used in the sense I have given in

* In the above figure, for 'Quaduct' read 'Quadrate,' and for 'Bill Duct' read 'Bile Duct.'

my recent papers on the post-embryonal history of muscle.* It would be well to restrict the term hypoplasia to express the condition of failure to develop of a part normally present, and then the word aplasia, which is often used synonymously with hypoplasia, would represent the suppression of parts which have developed.

I beg to thank Mr. C. D. Cooke for making the drawings here reproduced.

---

* *Anat. Anz.*, 1898-9. *Jour. of Anat. and Phys.*, 1899.

PROCEEDINGS

OF THE

University of Durham Philosophical Society

(Abstracted from the Minutes).

November 3rd, 1898.

(At the College of Science, Professor Lebour in the Chair.)

The following candidates were elected members of the Society :—

T. Beattie, M.D.
R. F. Betts.
C. W. Cairns, B.Sc.
S. H. Collins.
R. G. Hatton.
J. W. Hayward, M.Sc.
T. Hunter.
Miss A. Pallister.
Miss M. I. Peacock.
Miss K. A. Smith.
J. A. Smythe, Ph.D., B.Sc.
J. W. Thornton, M.Sc.
W. Thompson, M.A.
F. Wakerley.
J. Watson, B.Sc.

The following proposed alterations of Rules were adopted:—

Rule IV. Insert the words, 'but not more than four Vice-Presidents shall be re-elected in each year.'

Rule VII. Add 'Members whose subscriptions for the current year are unpaid shall not be entitled to receive the Society's publications; and those whose subscriptions are two years in arrears may be struck off the list of members by the Treasurer.'

The following officers were elected for the session :—

President:
The Very Rev. The Warden.

Vice-Presidents:

Rev. H. P. Gurney.
Dr. F. B. Jevons.
Dr. J. T. Merz.
Professor G. H. Philipson.
Rev. Dr. Plummer.
Rev. Canon Tristram.

Committee:

F. Baker.	W. Campbell.
Professor P. P. Bedson.	Professor R. Howden.
Professor G. S. Brady.	Professor R. A. Sampson.

The Committee was authorised to fill the vacancy caused by Mr. Turner's resignation of the Secretaryship.

Professor Brady read a paper 'On the Nature and Origin of Fresh-Water Faunas.'

December 15th, 1898.

(At the College of Science, Mr. Baker in the Chair.)

The following candidates were elected members of the Society:—

J. Cadman.	W. Maxwell.
C. Howson.	G. Mack.
J. Hall.	F. A. Wilcox.

It was resolved that in future 'Sectional Meetings' should be held in addition to the General Meetings. The Committee was instructed to consider and report on the arrangements for holding such meetings.

Miss Smith read a note on *Bulimus Ovatus*, a snail common in the Barbadoes, and remarkable in that it lays large eggs having hard shells. She also exhibited some shells and eggs of this snail, lent by Miss M. V. Lebour.

Professor Bedson exhibited some unusually fine specimens of the 'Vinegar Plant.'

Professor Potter read a paper on 'A Bacterial Disease of Turnips.'

Mr. Solomon mentioned that the turnips in question came from a trial field at Cockle Park, and that the disease had only appeared in those sections which had been treated with manure from the sheep house.

Mr. Meek said that the sheep in this house had been attacked by 'sheep rot.'

January 19th, 1899.

(At the College of Science, Mr. Baker in the Chair.)

On the report of the Committee the following sections were instituted :—1, Chemistry and Physics ; 2, The Biological Sciences ; and it was resolved that the Sectional Chairmen and Secretaries should be *ex officio* members of the Committee.

The following officers were elected :—

Chairman of Section I.	...	F. C. Garrett.
Secretary	...	T. Baker.
Chairman of Section II.	...	Professor Potter.
Secretary	...	A. Howson.

The Committee reported that Mr. Campbell had been elected Secretary, and Miss Smith was elected to fill the vacancy in the Committee.

The following candidates were elected members of the Society :—

D. R. Carrick.	D. McGowan.
J. Jacobs.	R. L. Treble.

Mr. Meek read a paper on 'The Post-Embryonal History of Voluntary Muscles in Mammals.'

Dr. Bolam gave an account of the examination of a number of beef teas.

February 2nd, 1899.

Chemical and Physical Section.

(At the College of Science, Mr. Garrett (Chairman) in the Chair.)

Mr. Baker read a note on 'The Estimation of Alkalies in Insoluble Substances,' with special reference to a modification of the Lawrence-Smith method.

Professor Louis referred to the method of decomposing the silicates with ammonium fluoride, and determining the alkalies as sulphates.

Dr. Smythe read a paper on 'The Terpenes,' giving an outline of the work of Baeyer, Wallach, and Wagner, and a short account of his own work on pinene.

February 16th, 1899.

BIOLOGICAL SECTION.

(AT THE COLLEGE OF SCIENCE, PROFESSOR POTTER (CHAIRMAN) IN THE CHAIR.)

Mr. Garrett gave a report of the work of the Geological Photographs Committee, and exhibited the following photographs :—

1 and 2. Syncline in the Great Limestone : Fourstones old quarry.
3 and 4. Shales, Sandstones and Coal in Prudham Sandstone : Prudham-stone quarry, Fourstones.
5. Great Limestone and Overlying Shale : Fourstones quarry.
6 and 7. Faults in Coal Measures : Crag Point, Hartley.
8. Faults in Coal Measures : Near Charlie's Garden, Seaton Delaval.
9. Apparent unconformity between the Geodic Limestone and the Cavernous Limestone : Frenchman's Bay.
10 and 11. Breccia Gashes : Marsden Bay.
12. Cannon Ball Limestone : Coast near Whitburn.
13 and 14. Local unconformity in the Coal Measures : Near Table Rocks, Whitley.
15, 16 and 17. Irregular bedding, with patches of Coal : Coast near Whitley.

The following Committee was elected :—Professor Lebour, and Messrs. Baker, Cadman, Campbell, Garrett, Woolacott, and Howson (Secretary).

Miss Smith gave an account of Ziegler's recent paper on 'The Cœlom Theory.'

March 2nd, 1899.

CHEMICAL AND PHYSICAL SECTION.

AT THE COLLEGE OF SCIENCE, MR. GARRETT (CHAIRMAN) IN THE CHAIR.)

Professor Stroud exhibited simple forms of apparatus for performing a number of experiments in Physics, and gave a demonstration. He also exhibited Wehnelt's electrolytic interrupter.

Mr. Thornton exhibited Hampson's original apparatus for liquefying air in quantity and explained the method of working.

March 23rd, 1899.

Biological Section.

(At the College of Science, Mr. Meek in the Chair.)

Dr. Bolam exhibited several new physiological instruments.

Mr. Meek read a note on and exhibited an abnormal liver of a sheep.

May 18th, 1899.

(At University College, the President in the Chair.)

Mr. Collins read a paper on 'Native Methods of Manufacturing Sugar in India,' illustrated by a number of very beautiful photographs, which unfortunately cannot be reproduced here.

LIST OF MEMBERS OF THE SOCIETY.

* *Denotes an original member.*

*Armour, A. L.
*Armstrong, H. E., M.D.
Arnison, W. D., M.D.
*Ashton, A. W., B.Sc.
*Baker, T., A.Sc.
Beattie, Thomas, M.D.
*Bedson, Professor P. P., M.A., D.Sc.
Betts, R. F., B.Sc.
*Bolam, R A., M.D.
*Brady, Professor G. S., M.D., LL.D., D.Sc., F.R.S.
*Brown, G. M.
*Bullerwell, J. W., B.Sc.
*Burkhardt, G. H., M.Sc.
Cadman, J., B.Sc.
*Cairns, Mrs. C. W., B.Sc.
Cairns, C. W., B.Sc.
*Campbell, William, B.Sc. (*Secretary*).
Carrick, D. R., A.Sc.
*Caunt, G. W., B.A.
*Chaplin, G. P., B.Sc.
Collins, S. H.
*Cooper, J., B.Sc.
*Dale, Sir David, Bart., D.C.L., D.L.
Dixon, J. G.
Dodds, R., B.Sc.
Dunn, J. T., D.Sc.
Ellershaw, Rev. H., M.A.
Elsdon, E. J., A.Sc.
Fletcher, Rev. Mark, M.A.
*Fowler, Rev. J. T., M.A., D.C.L.
*Garrett, F. C., M.Sc. (*Secretary*).
*Gravell, John.
*Gray, W. R. H., M.A.
*Gurney, Rev. Principal H. P., M.A., D.C.L. (*Vice-President*).
*Hacking, Thomas.
Hall, G., B.Sc.
*Hallaway, R. R., B.Sc.
Hardie, T.
Hatton, R. G.
*Havelock, T. H., B.Sc.
*Heawood, P. J., M.A.
Hilton, W. K., M.A.
*Howden, Professor R., M.A., M.B.
Howson, C.
Hunter, T.
Jacobs, J.
*Jessop, Professor C. M., M.A.
*Jevons, F.B., M.A., D.Litt. (*Vice-President*).
*Kitchin, The Very Rev. Dean, M.A., D.D. (*President*).
*Knight, R.
Laws, A. R.
*Lebour, Professor G. A., M.A., M.Sc.
*Linton, A., B.Sc.
*Louis, Professor Henry, M.A., A.R.S.M.
Lovibond, J. L., B.A.
Mack, G.
*McConnell, W., Jun., A.Sc.
McGowan, E.
Male, F. J., B.Sc.
*Marston, Rev. H. J. B., M.A.
Maxwell, W. W.
*Meek, A., M.Sc.
*Mellanby, A. L., B.Sc.
Merivale, J. H., M.A.

*MERZ, J. T., PH.D., D.C.L. (*Vice-President*).
MUNDELLA, V. A., B.A., B.Sc.
*MURRAY, PROFESSOR GEORGE, M.A., M.D.
OLIVER, PROFESSOR THOMAS, M.A., M.D.
PALLISTER, MISS A.
*PATTERSON, R. J., M.Sc.
*PEARCE, REV. R. J., M.A., D.D.
PEACOCK, MISS M. I.
*PERCIVAL, A. L., M.D.
*PHILIPSON, PROFESSOR G. H., M.A., M.D., D.C.L. (*Vice-President*).
PHILIPSON, W., A.Sc.
*PLUMMER, REV. A., M.A., D.D. (*Vice-President*).
*POTTER, PROFESSOR M. C., M.A.
PROCTOR, A. H.
*REDMAYNE, R. R., M.A.
*RICHARDS, W., A.Sc.
ROLLIN, C., B.Sc.
SALKELD, C., B.A., M.B.
*SAMPSON, PROFESSOR R. A., M.A.
*SHAW, SAVILLE, M.Sc.
SOLOMON, F. O.
*SOMERVILLE, PROFESSOR WILLIAM M.A., D.Œc., D.Sc.
*STOCKDALE, H. F.
*STROUD, PROFESSOR HENRY, M.A., D.Sc.
THOMPSON, W., M.A.
THORNTON, W. M., M.Sc.
*TODD, J. J., A.Sc.
TREBLE, R. L., B.Sc.
*TRISTRAM, REV. CANON, D.D., F.R.S. (*Vice-President*).
TURNER, G. G., M.B.
*TURNBULL, W., M.B.
*URWICK, W. E., M.A.
*WADE, THOMAS.
WAKERLEY, F.
WALKER, REV. DAWSON, M.A.
WARD, H., M.A.
WATSON, J., B.Sc.
*WATSON, F. B., M.A., B.Sc.
*WEIGHTON, PROFESSOR R. L., M.A.
WELFORD, R., M.A.
WILCOX, F. A., B.Sc.
WOOLACOTT, DAVID, M.Sc.
*WRIGHT, PROFESSOR MARK R., M.A.
*YOUNG, W. H.

UNIVERSITY OF DURHAM PHILOSOPHICAL SOCIETY.

Income and Expenditure Account. Session 1898-9.

Income.		£ s. d.	Expenditure.	£ s. d.
To Balance from Session 1897-8		15 8 11	By Postages...	2 9 3
,, Subscriptions at 5s.—			,, Clerical Assistance	1 4 3
1 Subscription, Session 1896-7	£0 5 0		,, Expenses of holding Meetings	2 10 6
7 ,, ,, 1897-8	1 15 0		,, Printing *Transactions*	16 8 6
91 ,, ,, 1898-9	22 15 0		,, Miscellaneous Expenses	1 1 6
4 ,, ,, 1899-1900	1 0 0		,, Balance in Treasurer's hands	17 11 11
		25 15 0		
,, Sale of *Transactions*		0 2 0		
		£41 5 11		£41 5 11

Examined and found correct.

HENRY LOUIS, *Auditor*.

November 1*st*, 1899.

COPIES OF THE 'TRANSACTIONS' IN STOCK.

Vol. I., No. 1, 116 copies.
Vol. I., No. 2, 120 copies.

PROCEEDINGS

OF THE

UNIVERSITY OF DURHAM

PHILOSOPHICAL SOCIETY.

VOL. I., PART 4.—1899-1900.

NEWCASTLE-UPON-TYNE:
ANDREW REID & COMPANY, LIMITED, PRINTERS AND PUBLISHERS.

1900.

OFFICERS OF THE SOCIETY.

SESSION 1899-1900.

President:

THE VERY REV. THE WARDEN.

Vice-Presidents:

PROFESSOR P. PHILLIPS BEDSON, M.A., D.Sc.
REV. H. P. GURNEY, M.A., D.C.L.
F. B. JEVONS, M.A., D.Litt.
PROFESSOR G. H. PHILIPSON, M.A., M.D., D.C.L.
A. L. PERCIVAL, M.A., M.D.
REV. CANON TRISTRAM, M.A., D.D., F.R.S.

Committee:

R. A. BOLAM, M.D.
PROFESSOR G. S. BRADY, M.D., LL.D., D.Sc., F.R.S.
PROFESSOR R. HOWDEN, M.A., M.B.
J. T. MERZ, Ph.D., D.C.L.
PROFESSOR R. A. SAMPSON, M.A.
MISS K. A. SMITH, A.Sc.

Chairman of the Chemical and Physical Section:
PROFESSOR HENRY STROUD, M.A., D.Sc.

Secretary: T. BAKER, A.Sc.

Chairman of the Biological Section: PROFESSOR M. C. POTTER, M.A.

Secretary: G. POTTS, B.Sc.

Honorary Secretaries:

F. C. GARRETT, M Sc.
G. MACK.

University of Durham Philosophical Society.

Notes on Blow Guns.

By Professor Henry Louis, M.A.

I feel a certain amount of diffidence in asking members of a philosophical society to cast their thoughts backwards, even for a moment, to one of the more primitive toys of mischievous boyhood, though I venture to hope that some at any rate, may still remember the time when they took an interest in pea-shooters. I purpose to-night to submit to you a few facts, chiefly from my personal observation, relating to what may be called the archetype of pea-shooters, namely the blow-gun as used by certain savage tribes. I do not mean to assert that the pea-shooter is a direct, though degenerate, descendant of the blow-gun, because I am inclined to think that it is more probably an independent invention. There would, of course, be nothing unusual in finding that the weapon of one race or age has come down to be the toy of another, for this is by no means uncommon; the sling and the bow and arrow are examples of such degeneration that will immediately occur to every one, and several others could be produced. Obviously they are more likely to occur in ballistic weapons, where there is more room for inventive development and for the displacement of old types, than in any other. I cannot, however, find any record that the blow-gun was ever used as a weapon in Europe; I do not know whether it was known in any form—either as a weapon or as a toy—in the

classical ages, and hope to elicit some information on that point from members of the society who may have devoted special attention to classical weapons. There are, moreover, certain reasons why one would expect blow-guns to be confined to inhabitants of tropical countries. In the first place the blow-gun is useless as a weapon, either of the chase or in warfare, unless the dart which it carries is poisoned, and that, too, with a poison of considerable virulence. Its limited range would, moreover, make it unsuitable for use in the open; it is only of value in the densest jungle, where its user can approach unperceived within easy range of his intended victim. For the latter reason also, it is found to be generally abandoned by tribes that possess sufficient mechanical skill to enable them to design and manufacture weapons capable of longer range, this mostly implying a considerable amount of metallurgical skill, as such weapons are for the most part tipped, at any rate, with iron or some other metal. The blow-gun, therefore, will only be found in races (often nomadic) whose knowledge of the arts is still quite rudimentary, but whose intelligence has been sufficiently cultivated to render them experts in toxicology. It may be noted in passing, how far more advanced savage tribes are in destructive than in curative medical knowledge; that, at any rate, has been my experience amongst them.

I have only met with the blow-gun in two places—amongst the aboriginal inhabitants of the Malay peninsula, and amongst the Indians of Central South America. It need hardly be said that in each of these instances it must have been a case of independent original invention, in spite of the numerous and striking points of similarity between the weapons as used by these nations so far remote from each other. I have never met with the blow-gun anywhere in Africa, and believe that it is equally unknown in Australia or North America.

In Malaysia, the blow-gun is not used at all, or only quite exceptionally for amusement, by the Malays themselves, its use being confined to the aboriginal tribes. In the Malay peninsula it is only found among the aboriginal inhabitants, known to the Malays by the various names of Sakei, Jacoon, Semang, Benua, Orang Utang, Orang Bukit, etc. These names may all represent one or several, more or less closely allied tribes, who inhabit the more remote, wilder, and all but inaccessible portions of the interior of the peninsula; they were apparently the original inhabitants, but were dispossessed by the Malays, who conquered the country and drove back its primitive inhabitants into the wildest parts of the interior, where the Malay was either unwilling or unable to pursue them. They are far lower in the scale of civilization than the Malay, are nomads living entirely on jungle produce, on roots, fruit, bark, leaves, etc., and by hunting and snaring birds, beasts, fishes and reptiles. Their clothing consists, for both sexes, of a skirt made from the inner bark of certain trees that is stripped off in sheets and beaten to make it flexible. They make rude ropes of certain vegetable fibres, but are unacquainted with even the simplest form of textile industry. They are equally deficient in metallurgical skill, and have no iron except what some of the more civilized have acquired from the Malays by bartering jungle produce. Their own weapons consist of spears fashioned from a strip or shoot of bamboo, the point of which is hardened in the fire, and the blow-gun. They build no houses or huts, merely erecting little temporary screens of branches and leaves to keep off wind and rain. But little is known of these curious people; centuries of persecution by the victorious Malay race have had the usual results, and they have become suspicious and treacherous. My first experience of the Sakei was meeting in the jungle some of the more civilized, who were on their way to a Malay-Chinese

settlement with jungle produce which they intended to barter. I happened to be the first white man they had ever seen, and on catching a glimpse of me they flung down their loads and tore off into the jungle like so many wild beasts. I was told that I was lucky that they had not first shot one or two poisoned arrows at me as a precautionary measure. Under such conditions it will be readily imagined that intercourse with the Sakei is not easy, especially as very few of them know any Malay. In the southern part of the peninsula, however, I came across some that had become more civilized, had lived for some years in the same district, and knew some Malay. From some of these I got the specimens of their blow-guns and arrows which are now before you. The blow-gun, which is generally known throughout the Malay Peninsula and Archipelago under the Malay name 'sumpitan,' is not, as generally stated, made of bamboo. The bamboo you see here is only an outer casing to protect the tube of the blow-gun proper. The latter is made from a species of reed, cut into lengths, which are slipped one into the other, the knot where a fresh segment of reed begins forming a kind of ferrule. A tube about seven to ten feet long is thus made, quite smooth inside. It is cemented into the bamboo by a little 'damar,' a semi-fossil resin found abundantly in the peninsula, and to the lower end a wooden mouth-piece is fitted. The arrow or dart consists of the rib of a palm leaf or the spine of certain palms brought to a fine point, with a short piece of soft light wood attached to the bottom end. When shooting with the gun, a dart is first put into the tube and then a tuft of silk cotton ('bombax' ?) pushed behind it to make a tight packing. A quick sharp puff, given in a peculiar way, though apparently without much effort, sends the dart flying at high speed. I have seen Sakei repeatedly strike a mark not larger than the palm of the hand at a distance of quite thirty yards, the

point of the dart going so hard into the bark of the tree aimed at, that it could not be pulled out without breaking. It would certainly have gone half an inch into a naked Sakei at that distance. It is said they can hit a bird or monkey on the top of the highest tree—a very good shot for a gun. Each dart is carried in a short piece of reed, a dozen or two being carried in a quiver of bamboo attached to a piece of cord made of vegetable fibre.

When hunting or fighting, the Sakei poison the ends of their arrows. It is not easy to get accurate information as to the poison used, but it seems to be a concoction consisting mainly of the juice from the Upas tree (*Antiaris toxicaria*). The Malay name for this tree is Ipoh, but as the same word is used generically for vegetable poisons, it is quite probable that several poisonous plants may be confused under one name. It seems that the juice of the root of the Tuba plant is also used. This latter plant is well known (it is stated to be the *Cocculus Indicus*) on account of its being used by Malays in order to stupefy the fish in a stream. The poisonous concoction forms a thick treacly mass looking like opium, and with this the point of the dart is smeared for about three-quarters of an inch. The Sakei sometimes notch the dart just below the smeared end, so that the point shall break off and remain in the wound; otherwise it could be pulled out immediately, before the poison had time to act. It seems that the poison must be tolerably fresh to be active, and even so its virulent effects appear to be greatly exaggerated. According to some authorities it produces bad wounds, but which are rarely fatal. In some experiments which have been recorded, a squirrel died in twelve minutes after being shot, and a fowl in two hours. It is said that the Sakei themselves declare that at its strongest three arrows will kill a man in one hour, and a tiger in three. It is said that in Borneo a man who received two darts, in an encounter with Dyaks, died

in two hours. I have never, myself, seen the poisoned darts used, but am inclined to think that their effects are not often fatal; my opinion is strengthened by the fact that I have been told on two separate occasions by old Sakei in different parts of the peninsula, as a little-known secret, that Indian corn chewed and placed in the wound forms a complete antidote. I am convinced that the Sakei who told me this thoroughly believed in its truth, but it seems pretty clear that such a remedy could not be efficacious against a really virulent poison introduced into the system by means of one of these darts.

Even if we credit the Sakei dart with being as poisonous as the natives claim it to be, it is obvious that it is ill-suited to the purposes of warfare; a dart that will prove fatal in an hour's time, but that does not cripple an opponent at once, is just the worst possible weapon for fighting purposes, however useful it may be in hunting. Bearing in mind that gunpowder was known from an early date in the Far East, and that the Malays had a kind of artillery probably before we had in Europe, it is not astonishing that the use of the blow-gun in the East has been confined to the least civilized tribes and has never been extensively adopted.

I regret that my information about the blow-gun in South America is far less precise. I only saw it there once in the hands of some Indians in the Eastern Cordilleras of the Andes, not far from Mount Pichincha; these Indians were shooting humming-birds for a trader in Quito, and used a blow-gun about six feet long, from which they shot pellets of soft clay, thus knocking down the humming-bird without damaging the plumage. These Indian tribes can speak very little Spanish, and the information I got about the blow-gun was at second-hand from men who knew these tribes well. They said that the blow-guns were made by scooping a groove out of a piece of straight grained wood (I believe of a palm tree), the tooth of an animal being used for this

purpose. Two such pieces were bound together and cemented. I was also told that they used poisoned darts, the poison, according to some, being curare, according to others derived from the poison bags of snakes. The locality whence these Indians came was near some of the sources of the Amazons, and it is noteworthy that both Bates and Wallace mention the use of these blow-guns in their works on the Upper Amazons, the former calling it 'Tarabatana,' the latter 'Gravatána.' Bates describes its construction in exactly the same way as it was described to me; he says it is a tube, nine to ten feet long, made from strips of wood scooped out by means of the teeth of animals; the strips of wood are wound tightly with strips of bark of a climbing palm and made tight with wax. It is then fitted with a cup-shaped wooden mouth-piece. Wallace, on the other hand, saw blow-guns made from the stems of palms, by pushing out the interior pith and rubbing the bore clean with a bunch of roots of a tree fern. This hollowed stem is then pushed into a larger stem, the object being, according to Wallace, that any curve in the one may counteract any curve in the other. It seems more probable that, as in the Sakei 'sumpitan,' the outer tube is intended merely as a protection to the inner one. Wallace states that these blow-guns have a conical wooden mouth-piece fitted to them, and that they are sometimes bound spirally with the leaves of a creeper. Bates states that the darts are made from the rind of the leaf stalk of certain palms, Wallace from the long spines of a tree; both state that a tuft or mass of silk cotton (a light silky fibre covering the seeds or lining the seed-vessels of certain trees) is attached to the bottom end of the dart so as to fit the tube fairly, and that the point is poisoned with urari, curare, or woorali, which is said to be obtained from the *Strychnos toxifera*, a tree which appears to grow only on the Upper Amazons. This poison is said to be very deadly, causing, when fresh, almost instant death

in such large animals as a tapir. Both authors agree that the range of the weapon is fifty or sixty yards, or quite as much as an ordinary gun. Of course the descriptions of these two naturalists refer to regions many hundreds of miles apart, and there is no doubt but that the Indians of each district employed whatever material, obtainable readily on the spot, was most suitable to their purpose. It is noteworthy that both writers speak of the Indians as having bows and arrows, as well as blow-guns; it seems, from what can be learnt, that the latter are used only in hunting and less, if at all, in warfare. It is at the same time clear that the poison used by the South American Indians is far more deadly than that of the Sakei; whilst it is interesting to have note that in both countries vegetable poisons seem to have been exclusively used, as far as definite information on the subject goes.

I have ventured to submit these notes to the Society partly in the hope that some account of these little-known weapons may prove of interest to some, and still more in the hopes of eliciting some information on the many problems in ethnology, history, medicine, and botany, which their study presents.

Notes on the Theories of Preformation and Epigenesis.

By T. E. Hodgkin, B.A.

[Read November 30th, 1899.]

The subject to which I wish to call your attention to-night is an enquiry into the processes of growth and development. How is it that organisms, both plants and animals, grow up from small and even microscopical eggs into larger and highly differentiated bodies? The marvellous rapidity with which growth takes place seems a very small problem compared to that much greater one, how is it that growth takes place at all, and that from a single cell an individual exactly resembling its parents develops? This problem is one which in all probability we shall never completely master, as we never expect to discover with any amount of finality how organic life is derived from inorganic. I intend to point out to you the two antagonistic theories that scientists, from Aristotle's time to Schwann, have advanced as a solution of the process by which growth occurred. These are the theories of preformation and epigenesis. We shall see difficulties beginning in last century and increasing in this, which culminated in the abandonment of the theory of preformation in its first crude form. Meanwhile the cell theory and our knowledge of the ultimate particles of matter have altered our conception of epigenesis, so that instead of the two theories, as originally stated, we find in the latter half of this century their descendants. His's theory of germinal localization and Weismann's germplasm range themselves on the side of preformation, whereas Nageli's idioplasm theory,

together with the view that cytoplasm is isotropic, supported by Pflüger, De Vries, Driesch and Hertwig, may both be called adherents of epigenesis. The first attempt to solve the knotty questions of development and growth was made by Aristotle, who maintained that in the higher animals at any rate the formation of new organisms by the process of generation does not take place suddenly. It is not due to a sudden and simultaneous accretion of rudiments of the organs of the adults, nor by metamorphoses of some formative substance into a miniature which grows into the complete organism, it is rather by successive differentiation of a relatively homogeneous rudiment into the parts and structure which characterise the adult. This is the essence of the doctrine of epigenesis, and Aristotle was the first who sought to solve the riddles of growth and heredity by an epigenetic process of development.

The views of Aristotle were not accepted by Galen and Hippocrates, nor did the medical world of classical times lend them its support. They believed all the organs, brain, liver and heart were formed simultaneously from vesicles.

It was in the seventeenth century and in our own country that the views of Aristotle were first recognised by Harvey, who in a work of immense importance for science gave the whole weight of his support to keenly advocating their correctness. Even if Harvey were not famous for discovering the circulation of the blood, he would be so for the work he did in pressing the claims of epigenesis. He tried to prove experimentally that with the higher animals growth is a gradual process of new development, of complex from simple. From his observations on growth he thought that the blood was the primal generative tissue, and that one of the functions of the blood was to cause the original formation and afterwards the growth of all the organs.

It was a great misfortune for science and the progress of views on development that Harvey should have based his

perfectly correct deductions on this incorrect observation. For it was largely due to this error that for a hundred years or so, the theory of epigenesis was thrown into the background, and the counter theory of preformation brought forward. Perhaps it was no real misfortune. The antagonism between the two views created a healthy partisanship, and we may hope the delay in arriving at a decision between the two theories was more than over balanced by the greater clearness in our views, which the struggle has produced.

At the end of the seventeenth century, Malpighi, while watching a developing hen's egg, saw that the chick's body was formed before any signs of blood appeared.

This proved Harvey to be wrong in supposing the blood was the generative part which gave rise to all organs in the body. This observation of Malpighi was quite correct, but unfortunately he thought it disproved Harvey's ideas of epigenesis and that therefore preformation must be the true way in which organs were formed.

The theory of preformation, or as it used to be, and is still more often called, evolution—though for clearness sake it now seems wiser to retain the more distinctive title—the theory of preformation is this, that growth and development are simply an unfolding of something that is already fully differentiated on a minute scale.

The preformationists compared a reproductive cell to the bud of a tree. From the outside it looks simple, but dissection shows that it is not so, and discloses inside the bud a mass of leaves and stalks, a little piece of stem and growing point, bracts, stipules, flowers, all may be there, even small buds of the following year. But these are carefully folded up, and the leaves doubled upon themselves, without there being a trace of them visible to outside world.

In the spring the sun and the rising sap cause the bud to burst and all its contents to unfold and grow. But this growth is in size and not in differentiation.

This analogy between preformation and a typical bud is not perfect, for every year new buds are formed and differentiation of tissue must be taking place inside them. This is not what the preformationists held in regard to reproductive cells. Their view was that nothing new was ever differentiated or developed, it was only an unfolding, development and differentiation were done once for all at the creation of the world, and it was that differentiation which continued to unfold or evolve.

This view of growth must necessarily apply to reproductive and regenerative cells of all types and classes, not merely to sexual reproductive cells, but also to asexual ones and the cells by which regeneration occurs. The theory holds too for the vegetable kingdom as well as for the animal. So the amount of preformation that must have been done at the creation is no doubt large. To me it seems that there are two great reasons which probably led to this once generally accepted theory of preformation. One of these is the fact that in a very short time counted by only a few days, or in some cases even hours, nature is able to produce fully formed and highly differentiated organisms out of germs, which epigenesis supposes to be homogeneous. The question at once presents itself, is it possible for this to occur, surely it is more likely that the apparently simple germ is simple only in outward appearance, and is in reality most complex, though invisibly so. This was and is a great difficulty to those who advocate the theory of epigenesis. The second reason is the very great difficulty there is in combining the well-known facts of heredity with the theory of epigenesis. The characters of any species remain for practical purposes constant and unchanged through vast numbers of generations, and yet the only link through which these characteristics can be handed on is a single germ cell, which the epigenesist would have us believe to be perfectly homogeneous. If it is hard to understand how heredity can

take place through the agency of a single cell differentiated as the preformationists maintain is the case, how much harder the problem is when the connecting link is a homogeneous and simple undifferentiated cell.

This account of preformation has led us some way from the historical aspect of the two views of development. It was Malpighi's observation upon a developing hen's egg, that the formation of the chick's body preceded that of the punctum sanguineum, which led him to believe that the chick's body really existed in the egg before incubation—led him in fact to preformation.

Bonnet followed on the lines of Malpighi and so did all the great scientists of last century with one splendid exception—Wolff. Those who believed in preformation included amongst their number Schwammerdam, Loeuwenhock, Haller and Spallazani.

It was Bonnet who pressed home the theory of preformation to its logical consequences.

The true preformation view entails the belief that every egg contains inside it the eggs of the next generation, these the eggs of the grandchildren generation, and so on; the eggs of every generation being contained in those of their parents.

This pill-box theory of germs, commonly called Bonnet's theory of emboîtement, because he pictured the germs encased within each other, one within the next, like a series of wooden puzzle boxes; this strange theory quite staggers the imagination. Bonnet himself in later life felt its absurdity and renounced it. Supporters of Bonnet's theory even went to the length of calculating how many human germs were present in the body of our ancestral mother, with the result that Eve was accredited with the possession of more than 200,000 million germs. It needs no small amount of faith to believe in a theory which produces such statistics!

Another of Bonnet's views was that growth took place by a process of intussusception. The object of fertilization

and incubation is to cause the preformed germ to absorb nutrient matter, which is deposited between its interstices. The full grown animal or plant consists of two parts, the preformed embryo and the nutrient matter it has absorbed by intussusception. When death occurs, these nutrient matters are withdrawn, and the body sinks back again to the germ stage. This means that true death never occurs, the germ is immortal, life is only its unfolding and expansion till it is visible to us, and death is a folding up again. This pretty suggestion we cannot accept, it is dependent on the theory of preformation, and must be abandoned with it. In the annals of last century the one great name which we find ranged on the side against preformation is that of Wolff. In 1759, from embryological work, he showed that the preformation doctrine was an untenable one. None of his contemporaries, however, were influenced by his work, and the preformation theory continued its popularity. The significance of Wolff's doctrine lies in this, that it rejected the purely formal doctrine of preformation because actual observations were against it. Wolff thereby freed research from the bonds of prejudice and opened the path of experimental research along which science has made such great strides in our day. The difficulties with which preformation had to contend may be said to commence when Bonnet showed that his theory of emboîtement was a necessary outcome of the extreme preformationist's view.

When it was discovered that fertilization consisted in the union of an ovum and a spermatozoon, a second grave difficulty presented itself to the followers of preformation. Here are two separate cells, from whose fusion the egg containing the preformed but invisible embryo arises. In which of these cells, before fusion, does the embryo live; in the spermatozoon or the ovum? Some preformationists thought in the one, and some in the other. For a hundred years this controversy raged, it ceased only with the very existence of the preforma-

tionist theory, which in its old form was abandoned at the beginning of this century.

The cell theory was first formulated by Schwann in 1839. He recognised that the egg was nothing more than an ordinary, though somewhat specialized, cell having at times a cytoplasmic portion in size out of all proportion to the nucleus. In 1841 Kolliker placed the male cell on exactly the same footing. And in 1858, Virchow proved that an uninterrupted succession of cell divisions, extending from one generation to another, was the method by which the spermatozoon as well as the ovum was formed from the tissues of their parent.

These discoveries have taught us to regard every organism in terms of its constituent cells; we know the ova and spermatozoa are simple cells, and that the developed organisms are only combinations of a very large number of cells, which are produced by the repeated divisions of the fertilised egg cell. If this theory be true, as experience has so far shown it to be, we cannot regard an ovum or spermatozoon as anything but a detached portion of cellular tissue of the parent. It is impossible to suppose they contain fully differentiated and preformed embryos.

The two other facts we have learnt from the cell theory are first, that the cell is no simple body but a most complex one, and is in fact a microcosm, and secondly, that the process of fertilization is always accompanied by the exact fusion of male and female nuclei, the equivalence of the male and female nuclear masses and of their distribution amongst the daughter cells.

A third difficulty to the complete acceptance of preformation comes from our knowledge that matter is not amorphous and cannot undergo division to an indefinite extent. It is made up of indivisible atoms and molecules which can only be divided with the loss of their individuality. As matter is not divisible to an indefinite extent, it is

impossible to have a descending series of germs, one enclosed in another, as Bonnet's doctrine of emboîtement implies, without sooner or later coming to a fullstop. In fact as soon as there is left only one molecule to represent the germ, division can go no further; long before this stage could be reached all idea of calling the germ representative 'preformed' must have vanished. Thousands and millions of molecules are more probably needed to form a single germ; unless the theory of emboîtement be true, we are driven to assume that at some stage epigenesis occurs, and that a complex differentiated body is formed out of simple homogeneous matter. It is quite possible, and even probable, that a germ is in a certain sense preformed in the single-cell condition, and that this differentiation becomes visible during growth by a process not altogether unlike an unfolding. But it is equally certain that the germ cannot always have been preformed and at some period must have been epigenetically developed from simple homogeneous matter.

Our knowledge of atoms and molecules also alters the views we must hold about epigenesis. The differences between two germs must lie in difference of molecular structure and arrangement, and this is approaching a molecular preformation.

Let us then turn our attention to the more modern aspect of the processes of development and growth.

So much has been done in the last twenty-five years by research, both historically into the structure of cells, and also by experimental changes and malformations on developing germs, that we are now supplied with a fairly large array of facts on which to base our views of development; and any hypothesis we may frame as to the process of development must be in harmony with such experimental facts.

Though we have left the theories of preformation and epigenesis behind, we find that modern theories divide themselves on somewhat similar lines and hence may be regarded as descendants of the older theories.

Among modern workers in this line of research, are found His and Weismann, who support theories of a preformation tendency, and Hertwig, Driesch and De Vries all ranging themselves on the side of epigenesis.

The first attempt to place the processes of heredity and reproduction on a satisfactory basis was made by Nageli.

Before his time the cell was considered the unit of transmission. Nageli suggested smaller units, idioplasm and trophoplasm, of which all cells were composed.

Idioplasm is that part of the cell which actively directs its growth and transmits the hereditary qualities.

Trophoplasm is the more passive part being the nutritive plasm to serve as food for the idioplasm.

This suggestion had not long been made by Nageli, before four biologists independently confirmed it, by recognizing that the nucleus fulfilled functions imputed to the hypothetical idioplasm. Hertwig, Kolliker, Strasburg and Weismann were all led to the same conclusion by noting:—

1.—The equality of male and female nuclei in reproduction.

2.—That regeneration of low organisms never occurs unless a nucleus is present.

3.—That in fertilization certain elaborate precautions are taken to prevent a summation of the hereditary masses.

Let us examine the first of these reasons, the equality of male and female nuclei in reproduction.

That offspring bear the character of both parents in about equal proportions needs no proof. If then the father's qualities are transmitted to as great an extent as the mother's, there must be found some portion of both generative cells which is of equal size. In some cases the female reproductive cell is 100,000,000 times larger than the spermatozoon, yet even here the nuclei of both cells are comparable in size, though no other part of the cell is so.

The second reason that led to the identification of the nucleus as the active and important factor in reproduction and regeneration was derived from experiments on certain low organisms both animal and vegetable. Some of these can reproduce themselves by regeneration of small portions. One individual may be divided into a dozen or more portions each of which is able to grow up into a complete organism if it contain a nucleus. Portions without nuclei are found not to possess this power.

Thirdly—Elaborate precautions are taken to prevent a summation of the nucleus from occurring in fertilization. Daughter nuclei are formed by a division of the mother nucleus into two equal halves, except in preparing the nuclei for male and female reproductive cells; these nuclei are generally supplied with not a half but a quarter of that of the mother nucleus from which they develop.

The significance of this is readily seen when we consider that fertilization means the union of male and female nuclear masses. If in fertilization two nuclei of normal constituents were to unite and form one, without any reduction in the nuclear material, the nucleus of each succeeding generation would be of twice the size of that in the parent generation.

Careful precautions are taken to avoid this, by the nuclei of the reproductive cells containing only half the amount that normal nuclei do.

A further reason for accepting the nucleus as the important factor in heredity comes from Van Beneden and Boveri's observation, that the male and female nuclear matter is equally distributed among both of the first two daughter cells, and from these to the next generation. Thus in the full grown adult all parts have representatives of both parents.

On many grounds then we believe the nucleus is the governing factor in the transmission of hereditary characters and reproduction of new individuals. This limits our enquiry and converts it from a problem of the cell to a problem of the nucleus.

Of the modern theories tending in the direction of preformation there are those of His, Roux and Weismann, whereas the opinions of Nageli, Driesch, De Vries, and Hertwig are opposed to this and follow rather an epigenetic explanation.

In 1874, His advanced the hypothesis of germinal localization—that every portion of the egg cell, as soon as fertilization has occurred, may be mapped out into different areas, each of which develops into a corresponding organ in the adult.

This idea of prelocalization differs, but not very widely, from that of preformation. It appears to gain support from the very complicated architecture we know cell substance possesses, which led Fleming to think we might found a science of the morphology of inheritance by watching the microscopic changes in egg cells.

His's ideas are borne out by Ascideans and some other bilateral animals, where the median plane of the body is marked out from the beginning of cleavage, so that the first division apparently divides the animal into two lateral halves representing the two lateral halves of the adult.

Experiments of Roux made in 1888 on the eggs of frogs have a direct bearing on this point. After the first division had taken place in the developing egg of a frog, Roux killed with the point of a heated needle one of the two blastomeres thus formed, without injuring the other. The injured half died, but the uninjured half developed into a perfectly formed half-larva containing one medullary fold and one auditory pit, and not into a complete larva of half-normal size.

This seemed strong evidence that when a frog's egg had reached the two-celled stage, localization had taken place so that each cell represented a lateral half of the future embryo.

Later experiments have not entirely borne out this result of Roux's work on germinal localization.

And Pflüger has shown that even with the eggs of frogs different results can be obtained.

A frog's egg is composed of two parts, when floating in water the top half has a black pigment spot, and the colourless heavier half sinks to the under side.

After fertilization, a vertical cleavage divides the egg into two lateral portions, identified by Roux with the right and left halves of the future larva.

Pflüger arranged frogs' eggs so that they should lie horizontally in water. Division occurred vertically, that is to say, at right angles to the normal line of division, when the pigment end is uppermost. In spite of this division being at right angles to the usual one, a normal larva developed, proving no protoplasmic localization had taken place before division.

Some experiments by Morgan suggest the solution by which the contradictory results of Roux and Pflüger can be harmonized. Morgan found that after destruction of one blastomere, if the other remained in its normal position, a half embryo always resulted as described by Roux. But if the blastomere be inverted it may give rise to a half embryo or to a whole dwarf. He concluded therefore that the rotation of a single blastomere brought it into a state of equilibrium like that of an entire ovum, and a whole embryo was the result.

Roux and Weismann, both adherents of preformation, have propounded more elaborate schemes than His's prelocalization.

Roux suggested that nuclei are capable of two kinds of division, differentiating and doubling.

By a doubling division the nucleus divides into two equal halves similar to itself. By differentiating division, nuclei of different characters are produced, for it is supposed that the parent nucleus can separate some constituent granules to one daughter nucleus and some to the other. By this process the idioplasm is split into its constituent parts, which are gradually sifted and distributed to the various

nuclei of the embryo. As the power of settling whether a division be a doubling or a differentiating one rests with the nucleus, it is thus self-determining.

Weismann, the great supporter of modern preformation, agrees with Roux in accepting the existence of two sorts of division, doubling and differentiating. Development is not, according to Weismann, the growth of complex out of simple, but rather the reverse, a simplification of complexity.

The egg cell is the most highly complex stage, containing the determinants of all the future different parts of the body. And growth is a process of simplification, all the determinants being gradually sifted out to their proper destinations, so that the adult is the most simple stage, for no cell contains more than one determinant.

Amongst the many difficulties in accepting Weismann's ideas is the lack of any confirmation that the two divisions doubling and differentiating actually take place.

All cases of dimorphism, even the dimorphism of the sexes, is inexplicable by Weismann's theory as it stands, and a subsidiary hypothesis is put forward by him for this purpose, which supposes the presence of two germ-plasms, one for the male and one for the female, both of which are always present, though only one develops.

The difficulty caused by the alternation of generations is explained in a similar way.

It is only by such a suggestion that Maupas' experiments on Hydatina Santa can be explained.

If while the eggs were being formed in germaria, Hydatina are kept at a temperature of 27° Cent., out of a hundred eggs there are 97 males produced. But on the other hand, Maupas obtained 95 per cent. of females, by keeping the eggs at 14° Cent.

On Weismann's system every adult is represented by a definite number and arrangement of determinants in the egg cell, and so every kind of adult must be represented

in the germ-stage by its own germ-plasm, even male and female of the same species must have different germ-plasms. In Hydatina heat appears to cause the development of the male germ-plasm and cold of the female.

So far this explanation is fairly satisfactory, but when it is applied by analogy to polymorphous bees and ants, the complications grow alarming.

Many colonies of ants are characterized by having four different forms, which nevertheless all spring from the same parents. There are the fertile females or queens, the males and of sterile females both workers and soldiers.

If the polymorphous forms were distinctly and clearly marked and not subject to variation, Weismann's explanation might perhaps still be applied to them. He would say the four different forms are represented by four different germ-plasms in the egg cell, one only of these develops and three remain dormant. Emery has proved this explanation to be impossible, by producing a series of intermediate forms between these four well-marked extreme ones.

Ants when they wish to rear a queen take an ordinary egg and supply it with specially rich food in great quantity, with the result that the ordinary egg develops into a queen. If warriors are to be reared similar eggs are taken, but the food for them is different, as it retards the development of of the sexual organs, and increases that of the head and jaws.

Emery's work was to change the food supplied to a developing egg after development had commenced. Thus he would take away the queen food before the queen had been fully developed and replace it with warrior food. The ant which grew up from such mixed diet was neither queen nor warrior but something half way between; by similar means he found he could produce more than twenty different forms of ants from the same eggs, by simply altering the conditions and food.

Weismann's explanation demands the existence of separate

germ-plasms for each of these twenty intermediate forms. And the eggs for ants must have always contained at least twenty different germ-plasms, and of these twenty germ-plasms four and four only have developed till the nineteenth century scientific experimenters gave the other sixteen a chance of showing themselves.

Doubtless the truer and simpler explanation is that the external conditions are the factors determining which line development shall take. The eggs of a species of ants are all similar, and their development differs according to and in proportion to the different foods supplied to them.

Having seen the difficulties of the preformation views of Roux and Weismann we will now turn to the epigenetic side. Oscar Hertwig who in many ways is the leading figure in this field, maintains that the three chief factors of development are to be found in:—

1.—The growth and multiplication of cells as a moulding factor.

2.—The position of cells and their surroundings.

3.—The inter-relation of parts to the whole.

All these may be classed as conditions external to the cell; and herein lies the keynote of the differences between Hertwig and Weismann as representatives of their two schools. To Weismann all differences which arise during development are expressions of some differences which originate from inside the cell, that is, differences in germ-plasms. To Hertwig the external conditions surrounding the cells cause the differences in their development. As soon as one cell divides and forms a colony, every new cell must be subject to different conditions, which it is only reasonable to suppose produce some modifications in its development. Yet this factor is taken no account of by Weismann's school.

Herbst has done pioneer work in this field. The influence he has found external conditions to exert over development, is often most extraordinary. He found that when sea urchins

developed in sea-water containing an excess of potassium chloride, the ordinary larvæ were not formed, but instead of them, larvæ without calcareous skeletons and without a ciliated anus. These larvæ resembled far more closely the Tornaria larvæ of Balonoglossus. More profound changes even than this occur when sea urchins are in water containing lithium chloride. The blastula fails to invaginate and form a typical gastrula, but evaginates forming an hour-glass shaped larva of which one half represents an archenteron and the other half ectoblast.

Another curious case of the influence position, also an external condition, has upon hydroids is seen in Loeb's experiments. Many hydroids whose heads are cut off grow another by regeneration. Leob cut off both ends of the hydroid Tubularia and inverted it, when a head regenerated at the top end, where the foot used to be. A similarly mutilated Tubularia placed horizontally developed two new heads, one at each end.

Thus we seem bound to accept the conclusion that development, if it is to proceed on the normal lines, is dependent on normal conditions surrounding the organism, and when the conditions are altered abnormal development takes place.

This important factor is left out of account by Weismann's and Roux's hypotheses. Important it is, but far from being all important. External conditions may appear to modify to some considerable extent the lines on which development proceeds but they cannot change them fundamentally. We may be able to determine whether queens or worker ants be developed, whether males or females be produced in greater quantities, but we neither can nor do we ever expect to be able to produce one species from the eggs of another.

Hertwig with no inaptness compares an egg cell with all its myriad descendants that go to make up the body of a complicated metazoon to the first ancestor of man and all his descendants. In Adam we have a man living under the

simplest conditions with only one companion to share his world and doubtless fulfilling all the functions of life. Adam's descendants have built up the most complicated systems of politics, society, economics, which Adam had not, yet his descendants have derived all their characteristics and capacities from him, but these systems are the natural result of his descendants' increased numbers. Their increase has altered their external conditions and produced the complicated world we live in to replace the simple life of the garden of Eden. So with the cell, it also has the power of leaving descendants, and as the descendants increase in number, new conditions arise and new qualities are developed in the cells from their mutual interaction; but the qualities which depend on the presence of many cells cannot be reflected back, and called potentially present in the germ of the original egg cell.

The second great argument brought forward by the modern epigenetic school is based on the isotrophy of protoplasm. In 1891, Driesch, during some experiments on the eggs of sea urchins, managed to separate the cells of a developing egg by violent shaking. In the two-celled as well as the four-celled stage he succeeded in doing this. The blastomeres so separated developed normally each into a complete gastrula but of a dwarf size, half or quarter that of an ordinary gastrula, according to the period at which the cells had been separated.

This result differs from Roux's experiments on developing frogs' eggs, but the two can probably be harmonized in the manner previously suggested.

Another experiment of Driesch's was to subject the segmenting eggs of sea urchins to pressure, by this means he obtained flat plates of cells, with an abnormal arrangement of both cells and nuclei. Yet such eggs when pressure was removed developed into ordinary larvæ without any re-arrangement of the abnormally arranged cells.

The third and most conclusive experiment was made by Wilson on the Annelid Nereis. The sixteen-cell stage of this Annelid contains four large cells with big oil drops called macromeres and twelve micromeres or smaller cells. By subjecting one of these eggs to pressure during development Wilson obtained a sixteen-cell stage of eight macromeres and eight micromeres. Again on removing pressure the larvæ produced were normal except that they contained eight instead of four macromeres.

These few experiments proved that during the first few segmentations of a developing egg, no differentiation had taken place, and the protoplasm was isotropic. To sum up in Driesch's words, 'The blastomeres of the sea urchin may be regarded as formed of uniform material, and they may be thrown about like balls in a pile without in the least degree impairing thereby the normal power of development. The relative position of a blastomere to the whole determines what develops from it, and if its position be changed it gives rise to something different, in other words the prospective value of a blastomere is a function of its position.'

The balance of evidence seems to point pretty clear to protoplasm being generally isotropous during the first divisions of a segmenting egg cell at least. The work of Pflüger, Driesch and Wilson all support this. Roux's experiments on frogs' eggs at first appear to contradict it, but Pflüger's explanation of them shows that the protoplasm of the first blastomeres is isotropous, and only a rotation of one of the first blastomeres is required for a complete though small larva to develop.

There do, however, exist indications that protoplasm is not always isotropous; Morgan found that if a certain small portion of cytoplasm be removed from an unsegmented ctenophore egg, there is always produced an incomplete larva, with certain defects corresponding to the removed portion. Though the whole of the nucleus is there, the

cytoplasm seems to have undergone some localization which makes it an essential factor in development. It may be, some further research will bring these experiments into harmony with the general view that protoplasm is isotropous.

If they do not and the explanation of the difficulty lies elsewhere we shall probably find it to be, that differentiation does not occur in all animals at the same period so that in the single cell stage of the ctenophore differentiation may have been going on for a long time, whereas in sea urchins and frogs it has not at the corresponding period begun. We are too much in the habit of regarding the fertilization of an ovum as the very beginning of a new life. It is probably not so. The ovum is no simple and homogeneous body, but a most complex one with a long life history behind it before the act of fertilization took place.

I have tried to set before you some of the views that have been advanced to explain the processes of development; there are difficulties attending all of them, which I have also mentioned.

Starting from diametrically opposite poles of thought the two views of preformation and epigenesis have steadily grown nearer, till at the present time the differences between them are small, much smaller most probably than is generally supposed.

There are two great problems in development. How is it that the fertilized egg cells of all plants and animals, though apparently so much alike and with such small actual differences between them, have potential differences as wide as the two poles of the biological kingdom? If this is the problem that fixes our attention we shall certainly tend towards preformation, and find the solution of the difficulty in crediting the egg cells with a great deal more structure and dissimilarity than we have yet discovered.

The other problem is trying to follow the growth from simple to complex. Having once abandoned the emboîtement theory of Bonnet we are forced to admit that every

reproductive cell must be formed by the gradual accretion of molecules to one another, that the individuality of a reproduction cell can only be due to the complexity which results from simple parts coming together, and that growth at any rate up to the stage of the reproductive cell must be a gradual one from simple to complex.

If we keep our attention fixed on this problem, we shall certainly favor an epigenetic solution of the process of development. What then is the real difference between Hertwig and Weismann, taking them as representatives of the two lines of thought, is it not one of time? Does not the school of Weismann consider that all differentiation has taken place before the stage of the reproductive cells is reached, and that future growth is the unfolding of this complexity; whereas Hertwig maintains that differentiation is only beginning with the reproductive cells, and is due to the gradual influence of external conditions working with and on their inherent powers of division and growth. Do we not get a simpler idea by considering these processes to be both acting continuously?

Growth up to the stage of reproductive cells must have been gradual and much influenced by external conditions, so that by the time the egg cells are formed a great deal of differentiation and prelocalization may have taken place as Roux and Weismann maintain. But surely these factors will not cease here, the same processes which ended in the egg-cell will go on and further develop it under the influence of external conditions till the fully formed embryo results.

Can we not by thus moving our horizon below the cellular stage and by thinking of the processes of development as continuous below and above the single cell arrive at a clearer conception of development and harmonize to some extent the opposite modern views about it?

On Certain Colour Phenomena caused by Intermittent Stimulation with White Light.

By A. S. Percival, M.A., M.B.

[Read December 15th, 1899.]

I would claim your indulgence for bringing a physiological subject before this section, on the ground that the results of physical experiments are sometimes misinterpreted from a want of knowledge of the factors that contribute to the physiological sensation of colour. As an example I may take the anomalous conclusions reached by Forbes and Young as to the velocity of light. To this point I shall refer again at the end of my paper.

It is most important to bear in mind that colour is not a physical term, it is a purely subjective term; it merely expresses the sensation produced by a certain kind of stimulus on a certain kind of cell. Undulations of a frequency about 400 billions per second produce the sensation of red colour in most individuals, while undulations of a frequency of about 700 billions per second produce the sensation of violet. Before light waves reach a colour-seeing organ, they cannot be said to have colour. No one would apply the term nauseous to waves of the sea, though under certain circumstances they may induce nausea in certain individuals. In books on physics the expression red waves is often conveniently used as an abbreviation for a set of waves of that period that induces a sensation of red in most individuals. From this use of the word a general idea has arisen that waves of this specific period are the only ones that can give rise to the sensation of red. It is my object in the present

paper to show that the colour sensation of red may be produced by other means.

In 1888, Mr. G. N. Stewart made some experiments on the law of Talbot with reference to the sense of application of light. The law of Talbot may require some little explanation. An electrical stimulus applied to a muscle-nerve preparation may be so weak and last such a short time, that no contraction of the muscle results; but if such stimuli are thrown in at a sufficiently short interval, a muscular contraction or rather a tetanus results. In other words stimuli which individually are unable to produce a muscular contraction may be summated, if they are thrown in at a sufficiently short interval. The same is true of the stimulation of the retina, at least in this sense that stimuli, which when isolated, act for too short a time to produce a sensation, may do so if allowed to follow each other rapidly, without diminishing the length of each. This can only happen, however, when each stimulus produces some impression, which though not amounting to a sensation, is a step on the way to one.

The law of Talbot may be stated thus:—Once complete fusion has been reached no alteration in the intensity of the resultant impression produced by a series of flashes takes place, however short the time may be during which each flash acts, provided that the number of flashes in a given time and the length of each stimulation be always kept inversely proportional. Complete fusion of stimuli here is analagous to tetanus of muscle. Is there any limit of time below which the individual stimuli cease to affect the retina at all, even when the frequency of repetition increases in proportion to the diminution of the time during which each stimulus acts? in other words is the retinal tetanus a complete tetanus, however short the duration of each stimulus? This is not the same thing as to ask whether there is a minimum time during which a stimulus must act in order to call forth a sensation. Such a minimum there certainly is. It lies lower the stronger the light; and above this

limit and below another, the physiological intensity of an impression arising from a stimulus of given physical intensity depends upon the time during which it acts.

Mr. Stewart's experiments were carried out with a rotating plane mirror. A parallel beam of light was allowed to fall on the rotating mirror in a darkened room, and in this way a series of very short flashes of light were received by the eye of the observer. Fusion of the flashes was obtained by increasing the speed of rotation of the mirror. He found that for the shortest stimuli he was able to use, there was no noticeable change of intensity of the sensation, once complete fusion had been reached, however rapidly the mirror was rotated. Even for the faintest light no definite variation appeared, that is, there was no noticeable departure from Talbot's law even when the duration of each stimulus was less than $\frac{1}{8,000,000}$ second. If there be a minimum length of stimulus below which no summation takes place, it certainly lies below $\frac{1}{8,000,000}$ second for the weakest light.

In the course of this investigation Mr. Stewart found that when the mirror was rotated slowly but with gradually increasing speed and a beam of light was reflected from it to the eye, a series of colour changes was seen. A description of a typical experiment will best explain what was observed.

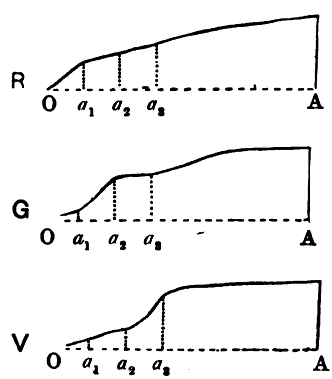
Mirror driven by gas-engine, petroleum lamp the source of light.
S (1) = 7 turns a second of the rotating mirror.
S (2) = 10 ,, ,, ,, ,,
S (3) = 17 ,, ,, ,, ,,

S (1) A dark green band appears at each side of the broad greenish yellow band, which represents the successive images of the flame of the lamp. When the intensity of the illumination was increased, the broad band became violet, bounded by two green edges.

S (2) Dark green edges have disappeared.

S (3) Faint but broad yellowish brown band at edges, and yellowish brown mottling over the rest of image. When the intensity of the illumination was diminished, the whole image became reddish brown.

As the result of numerous experiments it was found that for any given intensity of light, there is a rate of revolution of the mirror with which violet preponderates, with a higher speed green preponderates, with a still higher speed red. A decrease of illumination puts the whole phenomenon further forward and corresponds to an increase of speed, while an increase of illumination puts the phenomenon back to an earlier stage and corresponds to a decrease of speed. Hence as the intensity of illumination at the edges is not the same as in the middle, the colouration of the edges is different from that of the middle.



Now all these changes take place about or below the speed necessary for complete and steady fusion of the separate flashes. The idea at once suggests itself that the phenomena are connected with the different course of the curves representing the excitation of the three groups of fibres of the Young-Helmoltz theory.

The adjoining figure (Fig. 1) represents the curves given by Mr. Stewart for the red, green and violet sensations. The time during which the stimulus acts is measured along the horizontal axis, and the intensity of excitation is denoted by the ordinate of the curve at that point.

Suppose that the stimulus considered be that of white light. If it be of the comparatively long duration indicated by OA, the excitation of each of the primary visual fibres—red, green and violet—will be equal, as is seen by the equal length of the ordinates. Consequently the resulting sensation will be white. But if the stimulus lasts for a shorter period as Oa3, the violet visual fibres will be more excited than the green and much more than the red fibres. Consequently the resulting sensation will be blue or bluish-violet. If the period of the stimulus be very short as Oa1, the red fibres will have the preponderating excitation, and a red sensation will result.

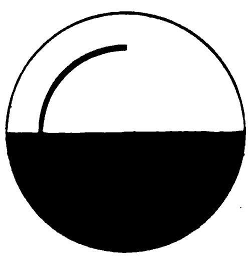


Fig. 2.

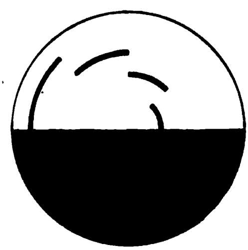


Fig. 3.

So far I have been practically only quoting from Mr. Stewart's paper. I must now attempt to give some reasonable explanation of the colours seen on this rotating disc.

Consider a disc half black and half white, the white part being marked throughout a quadrant by a curved black line. (Fig. 2). If the disc be now rotated counter clockwise at a speed of n revolutions per second, and the eye be fixed, a stimulus of white light will be received by the eye lasting $\frac{1}{4n}$ seconds. But owing to the persistence of the image the virtual duration of the stimulus will be $\frac{1}{4n} + a$ seconds, where a is a small fraction. When the speed of the mirror is such that $\frac{1}{4n}+a=Oa$ the line will appear red.

Now on reversing the rotation of the disc, the line appears blue. To explain this, we must draw attention to another physiological point.

When a uniform white surface is interrupted by a thin black line the black line is not so quickly taken up by the eye as a broad black surface. In this way the curved line will appear shortened to a greater extent than the after-image of the white extends over the black semicircle. Therefore in this case though the actual duration of the white stimulus is as before $\frac{1}{4n}$ seconds its virtual duration is $\frac{1}{4n}+\beta$ seconds where β 7 a.

On referring to the diagram we see that for a longer duration of the white stimulus Oa3, the violet ordinate is the greatest, and the green is next, so the colour between the two, or blue is seen. For a series of flashes following one another at an interval about that necessary for fusion, the ordinate corresponding to the length of each flash will still be the mean ordinate of the compound curve.

When the curved line occupies an intermediate position, the virtual shortening of the line will be less, as the white stimulus preceding it, will be less intense, for it has lasted during a shorter time. Consequently when a disc with four sets of lines as this (Fig. 3) is rotated four colours will be seen, red, stone yellow, dirty green and blue.

Colour Phenomena Observed by Forbes and Young.

The results of an elaborate series of experiments carried out by Forbes and Young on the velocity of light were published in the Philosophical Transactions of 1882. The method adopted was a modification of Fizeau's. It is unnecessary to describe the details. The source of light is placed behind a toothed wheel which can be rotated at a very rapid rate. The light passes out between two teeth, and is reflected from two distant mirrors placed nearly in the same line, but one

more remote than the other. When the toothed wheel is at rest the observer sees two stars side by side, of nearly equal brightness. When the wheel is in rapid rotation, the same thing happens for particular velocities. If after equality has been reached with one of these velocities, the speed be increased, the relative brightness of the stars changes. The star from the farther reflector gradually fades and finally disappears, the other reaching a maximum intensity to decrease in its turn. The star which has been first eclipsed will be the first to re-appear and will increase in brightness with the speed.

Now Forbes and Young observed that although white light was used, one of the stars was red and the other blue. Closer examination showed that when the brightness of either was increasing, its colour was red, and when its brightness was fading, its colour was blue. 'This they concluded to indicate a difference in speed of the waves of greater or less wave-frequency. Thus if the blue waves travel faster than the red, then the red waves will be eclipsed at a lower speed than the blue, so that as the speed of the wheel is increased the red waves will be first eclipsed and the fading image will appear blue. On the other hand, when the image is re-appearing after eclipse, the waves which travel slowest will be the first to gain admission through the adjacent tooth-space and the growing image will appear red. To test this point experiments were made with the red light and blue light of the spectrum formed by a prism, and from the average of the results the experimenters concluded that the blue waves travel about 1·8 per cent. faster than the red. This difference is so great, that in the absence of other support, the effects have not been generally accepted as due to a difference in velocity of the various waves, but it is surmised that the colouring is due to some extraneous cause not yet determined.'

The above is what Preston says in his theory of light.

Now will not Mr. Stewart's experiments explain this colour sensation? We have here an intermittent stimulation of the retina. The number of stimuli per second can be varied and so can the length of each; a small increase of the speed of the toothed wheel may extinguish one of the stars, *i.e.*, reduce its stimulation time to zero. A difference of speed of about 10 per cent. could produce an infinite difference in the brightness, and therefore in the time of stimulation. Accordingly the change in the velocity, so far as it affects the number of stimuli in a given time, may probably be neglected. We may consider in fact that the number of stimuli per second remains constant, while the length of each stimulation is continuously varied from zero to a certain finite maximum value, and from this value back again to zero. A glance at the diagram will show that as the stimulation time is increasing from O to Oa_1, the colour sensation produced will be red.

Further we know that the excitation takes some time to fade away, and that it fades away more slowly in the violet and green fibres than in the red. Helmoltz and Fechner have described a succession of colours in the after-image of a bright white object: 'the positive after-image goes quickly out of the original white through greenish blue into indigo blue, and then into violet.' Hence when the stimulation time is diminishing (or when the star is fading) the colour seen will be blue. For the excitation of the red fibres will rapidly fall away, while that of the green and violet fibres will for a time retain their pristine height. This is the explanation given by Mr. Stewart of the anomalous observations of Forbes and Young.

For monochromatic light Forbes and Young found that it required a greater velocity to produce equality with blue light than with red. Mr. Stewart suggests that this is due tc the fact that 'the minimum difference which can be appreciated is not the same for each colour. . . Two similar white

lights which seem equal when looked at through a red glass may not appear equal when viewed through a blue glass.' This does not appeal to me. If one of the similar white lights is composed of the two complementary colours, yellow and blue, and the other is composed of all the colours of the spectrum, of course they will appear of different intensity when viewed through a blue glass.

I have found that when this disc is illuminated by sodium light on rotation the colours seen are almost the same as when it is illuminated by white light. The sodium light must therefore excite all three primary fibres, though of course to a different extent. But the colour phenomena due to a difference in length of stimulation time are still apparent. Hence to explain Forbes and Young's observations I would make the following suggestion. Suppose that when red light was used, the two stars appeared of equal brightness when the speed of rotation was from 410 to 418 revolutions per second, say 414. Now as the excitation of the violet fibres falls away more slowly than that of the red fibres, it is obvious that when blue light was used, the stars will appear of equal intensity during a greater range of speed. Suppose that they appeared equal when the speed ranged from 410 to 430 revolutions per second. The mean of this is 420. Forbes and Young would naturally conclude that a greater velocity was required to produce equality with blue light than with red light.

I would make a similar suggestion to explain this curious phenomenon of the disc, when it is rotated rapidly. Fixing the attention on the line which appears red on slow rotation, it becomes bluish or mauve on rapid rotation, and the line, is continuous or fusion is complete. Presumably in this case the intervals of non-stimulation are so short that the excitation of the violet and green fibres have not had time to fall away, while the red sensation has entirely faded. The blue sensation will increase to a certain maximum, on which will be superimposed numerous little flashes of

red sensation. Hence a mauve colour is seen. Fusion of the blue sensations will occur sooner than fusion of the red sensations, or if such an expression be allowed, blue tetanus will occur at a slower frequency of intermission than red tetanus. Hence a mauve colour-sensation is produced.

The Pleasures of Boring.

By Professor G. A. Lebour, M.A., M.Sc.

[Read January 18th, 1900.]

There must always be in this country a certain number of rich men to whom the ordinary methods of spending the unallotted portions of their incomes appear unsatisfying and commonplace—men who each year find themselves with a few thousands in hand which they may give away, risk, or lose with a clear conscience. To re-invest this surplus in safe securities is prudent but tame. To gamble with it on the Stock Exchange is less tame, but has in it an element of business repugnant to many minds. To stake it at Rouge-et-noir or Baccarat is exciting, but objectionable to some on moral grounds. The Turf is equally exciting, but has similar defects. To spend it in charity, or in donations to educational and learned institutions, or to the funds of a political party, is Christian, philosophical, or loyal, as the case may be, but there is perhaps not much excitement to be got out of it.

It should, therefore, be a boon to such men to have pointed out to them a means of spending their odd thousands which would combine the prudence of the investor, the benevolence of the pious founder, the patriotism of the politician, with the exciting qualities of Epsom or Monte Carlo. Such a combination they would, I think, find in the great game of Boring.

Boring is too often regarded as merely a subordinate province of engineering to be entered into by professional men in search of water or minerals. This is its most obvious and utilitarian side. It possesses, however, most of the best characteristics of sport, and may be considered as

a game of many advantages. But, like yachting or racing, it is a game for the rich only. The chief qualities required in a player are: patience, temper, judgment, decision, firmness, alertness, boldness and money. Though it is an outdoor pursuit and any amount of physical exertion may be associated with it, it may yet be carried on, like chess, from one's armchair. It can be played alone, like Patience or Solitaire, or the players may be numerous, as at Vingt-et-un. The appliances made use of are varied and include instruments of many kinds. The ultimate object of these is in all cases to make a deep hole in the ground. To this object is in most cases added that of bringing up for inspection bits of ground from the depths bored to—soundings from the deep earth, in fact.

The way in which the game is played is as follows:—Let a player, A, furnished with the mental and pecuniary requisites already mentioned, acquire mining rights over a suitable piece of country, where no mining has yet been carried out, and where no valuable mineral has ever been proved to exist, but where it is possible that such mineral—say coal—may be present. This first step is of cardinal importance and necessitates a profound knowledge not only of the principles of geology but of the local peculiarities belonging to the particular region selected. Should A possess that knowledge—well and good; he will be in the position of a racing man capable of choosing his own stud. Should he, as is more likely, know little or nothing of such matters, he will, as so many owners of winners have done, engage an expert to make the selection for him. The choice of a competent, trustworthy, and sufficiently daring geological adviser will be the first test of A's capacity for the game.

The spot having been fixed upon, the probability that coal may be found within reasonable limits of depth must be ascertained by a careful examination of all the available

evidence. The method of boring to be adopted depends largely upon this, and many considerations will have to be weighed. There are the common boring rod and chisels, which are slow, but which can be carried down to almost any depth and which bring up the material of the rocks bored through in such a state that only after much experience can they be recognized. Then there is the American plan, much in vogue in the States for 'striking ile,' which pierces the earth's crust at a speed much greater than any other, but which brings up nothing but floods of liquid mud. Lastly there is Diamond Boring, more expensive than the others, and deliberate, but which brings up the actual rock in cores averaging some thirteen feet in length and varying in girth from the size of a church pillar to that of one's thumb.

Whichever apparatus is used, much will depend upon the master-borer, who may be looked upon as corresponding to the trainer of a racing stable. By his long-educated senses of touch and hearing he can tell the nature of the strata through which the chisel or crown is at any moment working its way. If incompetent, careless, or dishonest, he may declare coal-seams present which are not there, or he may exaggerate the thickness of unworkable seams, or, again, he may allow a good seam to pass unrecorded or may record it in inches instead of feet—just as an unfaithful trainer may depreciate the value of a good horse or appreciate that of a bad one.

Having, however, committed himself to a system of boring—the Diamond, probably—and assured himself, by means which his worldly knowledge will suggest, of the faithful co-operation of his master-borer, A rigs up his tackle and begins to bore.

During the progress of the hole—a matter of months usually—A, his expert, and his master-borer, will be incessantly occupied. Each foot of rock brought to the

surface will be closely scanned, the nature and thickness of each stratum will be registered and commented on. No particle of evidence tending to throw light upon the result can be safely neglected. Changes in preconceived views may possibly be brought about by the new knowledge derived from the boring. Sudden stoppages may occur from time to time. Soft beds may clog and impede the revolving crown of diamonds, the diamonds themselves may fall off and need replacing, the rods may break and great difficulty may be experienced in attempting to withdraw them, it may be necessary to devise tools for the purpose—much as surgeons have been known to invent special instruments in the course of a capital operation. Indeed the course of deep boring seldom runs smooth. Unexpected strata may be struck and fresh deductions must be formed from them, or the unexpected rocks occur, but lying in unforeseen positions. In such cases the cores probably show the amount of the dip clearly enough, but its direction may be doubtful, indeed the exercise of much ingenuity may be needed to solve this question—possibly a crucial one. As hundreds of feet of cores accumulate, and the diagnosed depth is neared, the hopes and fears of the race-course are felt by all concerned, and when success or failure is at length certain, as much pluck and stolidity are required of A as at the winning-post.

Success may mean the proving of a new and unthought-of coal-field, employment for large numbers of men, much additional wealth for A (in lieu of a Cup), and perhaps, as the obverse of the medal, the spoiling for ever of a beautiful landscape. Or else success may be of the *succès d'estime* kind, that is, the coal may be found, and A's acumen be proclaimed to the world, but too thin or too deep to be profitably worked.

Failure may, in the same way, be failure pure and simple, like that of the owner of the last horse. All the prognos-

tications of A and of his experts—trainers and jockeys—may be falsified and may be shown to have been based on insufficient knowledge or on wrong premises. This is hard to bear. But failure may be comparative. No coal may have been struck, but the information yielded by the boring as to the lie of the beds may be such as to render the favourable issue of a second trial almost a certainty.

At the worst, successes and failures alike add evidence of value for future undertakings, as it may chance that some unlooked-for mineral may be met with—for the element of chance is never absent from boring operations. What was not long ago the deepest bore in existence failed to find what it was put down to seek, but proved the quite unexpected presence of the thickest bed of rock-salt in the world.* A still deeper boring, on the other hand, only succeeded in proving an enormous thickness of unremunerative deposits.†

Such is the game of boring played by one. Played thus it may in the eyes of some seem lacking somewhat in emulation. But let two or more arrange to bore against each other, and all the elements of competition are at once brought into action. There is then no limit to the sporting possibilities of the struggle. Time, depth, presence or absence of coal, its thickness, its dip—these and numberless other points may become objects of rivalry and—if the savour of the forbidden fruit be absolutely needed—of betting.

The suggestion thrown out, then, is that active-minded men of means might find in boring for coal or other minerals an occupation at least comparable with the Turf in its mental requirements and in its cost, with prizes by the the side of which those offered by most other pursuits are insignificant, with spirit-stirring moments unequalled in the

* The Speremberg boring near Berlin, which passed through more than 3,000 feet of salt.

† The Schladebach boring near Leipzig.

life of the most fervent gambler, and with the single drawback (from the non-utilitarian point of view) that the winner by winning adds to the resources of his country.

As an example of this kind of problem which underground exploration by probe means, perhaps none can be selected more interesting or more important than that of the search for new coalfields in the southern portion of England. The conditions of that problem, eliminating all but the most essential, are as follows:

(*a*) The fact that coal in workable beds has so far been found to occur in Britain and in the neighbouring parts of the Continent only in the upper division of the Carboniferous Rocks or Coal-measures.

(*b*) That the Coal-measures hitherto known in those regions are in all but a very few cases known to be more or less incomplete as to their upper strata, and are therefore found either exposed to the day or covered unconformably by newer deposits.

(*c*) That the fragments of Coal-measures of the northern half of England, or so-called coal-fields, can be proved to have become separated by the removal, or denudation, of connecting masses of rock—in other words that they were once continuous.

(*d*) That this once-continuous large tract of Coal-measures has been proved not to have extended further south than a ridge of older rocks in existence at the time of the formation of the Coal-measures running across the island from Mid-Wales to South Leicestershire.

(*e*) That other fragments of Coal-measures or so-called Coal-fields occur to the South of this ancient ridge in South Wales, in the Mendips, at Bristol, at Burford Signett, and at Dover. That these Southern Coal-fields are in direct chain-like continuation of a similar series of Coal-measures fragments on the Continent in the Boulonnais, in the Pas-de-Calais and Nord Departments of France, in Belgium, at Aix-la-Chapelle, and in Westphalia.

(*f*) That in Western Holland another fragment or coal-field occurs a few miles to the north of that of Aix-la-Chapelle—that is to say north of a line drawn through the known South British fields and those of Northern France and Belgium.

(*g*) That a number of borings put down north of that line and south of the 'Mid-Wales to Leicestershire' ridge have proved the absence of Coal-measures at certain spots.

(*h*) That the South Wales and Belgian chain of coal-fields are known to occupy minor troughs of a more or less compound fold, along the main axis of which they are placed.

(*i*) That quite recent researches in Belgium have shown that there the coal-bearing synclinal fold is situated on the northern flank of a denuded ridge of pre-Coal-measure rocks and not along the actual summit of that ridge.

(*j*) That in South Wales the coal-bearing synclinal fold lies upon the southern flank of a denuded ridge of pre-Coal-measure rocks.

(*k*) That in the Mendips the Coal-measures are on the same side of that ridge but on the northern flank of a more southerly mass of older rocks.

(*l*) That the coal-bearing folds are most pinched up—laterally compressed—in the Belgian and French area, are less compressed both to the east and west, but become once more narrowed and squeezed in the extreme west.

(*m*) That where the lateral compression is greatest, there are found to be occasional inversions of strata and reversed faults approaching the horizontal, technically known as overthrusts, as in the Mendips, Boulonnais, Nord, and Belgian districts.

(*n*) That in the event of such inversions or overthrusts being present, the first strata of palæozoic age met with beneath the unconformable base of the post-Carboniferous rocks, if pre-Carboniferous in age, would afford no proof of

the absence of Coal-measures lower down. Two divisions of the palæozoic formations must be determined before such proof is possible.

(*o*) That the line of greatest height (or water-parting) of the great southern ridge of old rocks, where it is known, varies in level within limits represented probably by about 1,000 feet.

(*p*) That nowhere along the southern chain of coal-fields is the total thickness of Coal-measures present.

(*q*) That the post-Carboniferous beds which conceal large portions of the great southern ridge of old rocks are of varying thickness, the law of their variation being apparently this, viz.: a thickening of the clayey deposits and a thinning of the limestones on nearing the ancient ridge.

(*r*) That a certain correspondence has been found to often, but not in all cases, exist between the position of the flexures of the unconformable newer beds and that of the folds of the underlying rocks.

(*s*) That certain magnetic surveys of Britain have indicated, by means of lines of maximum intensity, the underground distribution of probable ridges of ancient crystalline rocks, which are presumably the basement structures in the upper portions of which the hidden Coal-measures are to be sought for.*

Given the body of evidence thus sketched out, it is required to find, in the South of England, beneath the post-Carboniferous beds:—

1st. The ridge of ridges of old rocks which they overlie unconformably.

2nd. Those portions of that ridge or ridges which are nearest to the present surface of the land.

* The sketch-map accompanying this paper shows the position of the principal bore-holes which have yielded evidence bearing upon the search for coal in the South of England. For further details, readers are referred to the writings of Godwin-Austen, Prestwich, Whitaker, Topley, Holmes, Harrison, Taylor, and Boyd Dawkins, and to several papers in the *Transactions of Geological Society of Belgium*.

3rd. Those portions of that ridge or ridges in which the folds are most compressed and where the Coal-measures have in consequence received most protection from denudation.

This is the problem to be worked out, and boring alone can give an answer to it. Every boring within the proper area, however unsuccessful in finding coal, helps to the final solution, narrows the field of enquiry, and renders ultimate success easier of attainment.

Explanation of Map.

The areas covered with close oblique lines are known coalfields.

That with lines wide apart is the region wherein Coal-measures *may* be met with at greater or less depths.

The numbered circles are borings; the black ones with a cross have reached the Coal-measures; the white ones have only reached rocks newer than the Coal-measures; the black ones without a cross have struck rocks older than the Coal-measures without passing through the latter, which are, therefore, absent at these spots.

No. 1.—Burford Signet: 1184 feet deep, reached Coal-measures.

No. 2.—Dover: 1113 feet deep, reached Coal-measures.

No. 3.—Ropersole: near Dover, reached Coal-measures as at Dover.

No. 4.—"Sub-Wealden" Boring at Netherfield: 1905 feet deep, reached Oxford Clay.

No. 5.—Rottingdean: 1285 feet deep, reached Lower Greensand.

No. 6.—Chichester: 1054 feet deep, reached Upper Greensand.

No. 7.—Portsmouth: 1037 feet deep, reached Chalk.

No. 8.—Southampton: 1317 feet deep, reached Chalk.

No. 9.—Meux's Brewery: 1146 feet deep, reached Devonian.

No. 10.—Kentish Town; 1302 feet deep, reached New Red Sandstone (?).

No. 11.—Cheshunt: 1010 feet deep, reached Devonian.

No. 12.—Ware: 837 feet deep, reached Silurian.

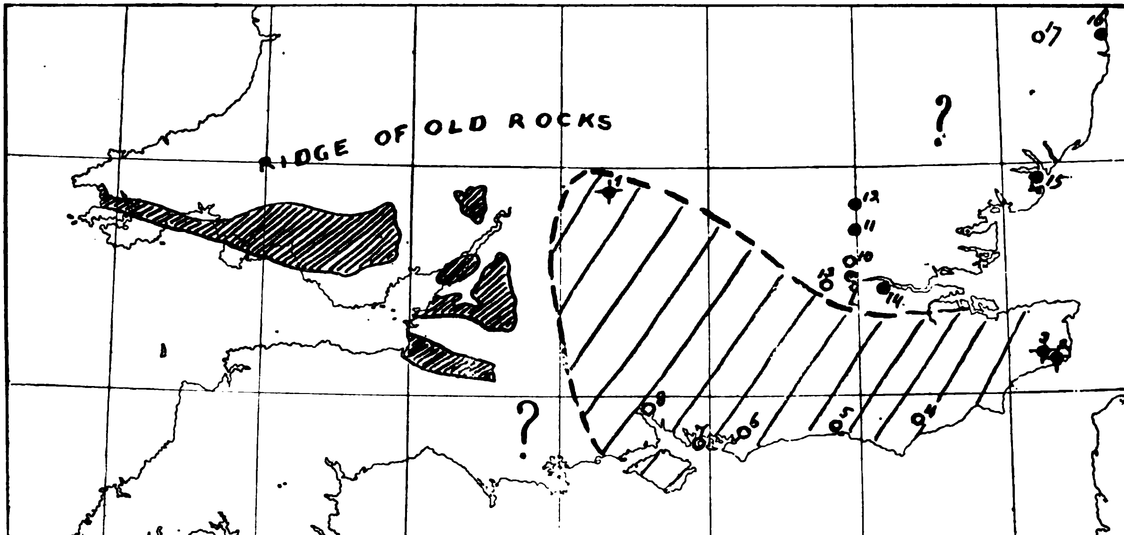
No. 13.—Richmond: 1447 feet deep, reached New Red Sandstone (?).

No. 14.—Crossness: 1075 feet deep, reached Devonian (?).

No. 15.—Harwich; 1098 feet, reached Carboniferous (Lower) Rocks.

No. 16.—Yarmouth: 583 feet, reached Chalk.

No. 17.—Carrow: 1206 feet, reached Gault.


RIDGE OF OLD ROCKS
?
?
1
2
3
4
5
6
7
8
10
11
12
13
14
15
16
17

Animal Life without Bacteria.

By F. C. Garrett, M.Sc.

[Read January 25th, 1900.]

It has long been known that the intestines of men and of other animals contain many micro-organisms, and there has been much speculation as to the part they play, and as to whether or no they are of service to their host; but until the last few years its great difficulty has prevented any experimental investigation of the problem. In 1885 Pasteur* expressed his belief that under sterile conditions—that is to say, in the absence of these organisms—life would be impossible; and he suggested that the question might perhaps be answered by hatching eggs in a germ-free incubator, and feeding the chickens with sterilised food. Nencki at once opposed this view,† arguing that the digestive changes are so much more rapid than the bacterial, that the assistance of bacteria is unnecessary; and that, moreover, the end products of their action are usually substances (such as the fatty acids, phenols, indol, hydrogen sulphide, marsh gas, etc.) which have no food value at all.

Here the question remained for ten years, and no serious attempt to answer it experimentally was made, until Nuttall and Thierfelder‡ commenced a series of most valuable experiments with guinea pigs, and showed that when the young animal is removed from its mother by the caesarian operation, and completely protected from all micro-organisms, it is possible for it to live and thrive. Immediately the guinea pig was born it was transferred to a sterilised apparatus;

* *Comptes Rendus*, C. 68.
† *Arch. fr. exp. Path. u. Pharmak.*, xx. (1886), 385.
‡ *Zeit. f. Physiol. Chem.*, xxi. (1895), 109; and xxii. (1896), 6

where beneath a glass bell-jar it was supplied with dry and germ free air, and with sterilised milk, and was kept under observation for the remainder of its short life. When about twelve hours old the little animal was able to sit up and to receive its first meal and from that time onward one of the experimenters fed it every hour through the day and the night; after eight days it was lively and in good health, and had grown larger, but the strain of the hourly attention told heavily on the investigators, and it was found necessary to conclude the experiment. The animal was therefore killed, and the intestines and their contents were examined bacteriologically, but without detecting any micro-organisms at all, thus proving that it is possible for an animal to thrive on a milk-diet without the aid of bacteria.

So much having been proved a fresh series of experiments was undertaken in order to find whether vegetable foods could also be digested under these conditions, and several guinea pigs were reared under the conditions already described, but were supplied with Albert biscuits as well as with milk. These experiments were even more successful than the first, two of the animals being kept in good health for fourteen days without the appearance of any micro-organisms in the intestines.

Nuttall and Thierfelder have thus shown that for the digestion of milk, sugar, fat and starch, the acid of micro-organisms is not required, though probably the digestion of cellulose is impossible in their absence.

Attempts were next made* to obtain eggs free from micro-organisms and to hatch them and bring up the chickens under sterile conditions but they were unsuccessful, and after a number of failures the authors concluded that in all probability the eggs were infected while still in the oviduct, and before the deposition of the shell. This conclusion has very recently been opposed by Schottelius,† who made during

* *Zeit. f. Physiol. Chem.*, xxiii. (1897), 231.
† *Archiv. f. Hygiene*, xxiv., 210.

1898 a large number of experiments with hen's eggs. Like Nuttall and Thierfelder he found very great difficulty in freeing the eggs from germs, but he showed that if the eggs are laid in a very clean nest, and are *at once* treated with solution of mercuric chloride (1 in 200) it is possible to cleanse the eggs and to obtain from them chicks which are also free from micro-organisms. Several chicks were obtained in this way and were kept alive for a number of days, but they did not thrive; as a rule they lost weight daily, grew steadily weaker, and none of them survived the eighteenth day. Bearing in mind that Nuttall and Thierfelder's guinea pigs though they appeared healthy gained weight but slowly.* Schottelius concludes that though without bacteria life is possible for a time nutrition proceeds imperfectly, and that they are really essential for healthy life. Against this it may be urged that in these experiments both the chickens and the guinea pigs were somewhat unfairly treated. The chickens were kept in a warm and extremely dry atmosphere which is quite a sufficient cause for their feebleness; while the guinea pigs in addition to being born in an unnatural manner and fed with *cow's* milk were deprived of their mother's protection and left entirely alone from first to last; Nuttall and Thierfelder have proved that these troubles are more than sufficient to account for the slow progress made by their little patients.

The disputed point has finally been decided by Levin† in a remarkably simple fashion. In the summer of 1898, Levin took part in Natthorst's expedition to Spitzbergen in 'the Antarctic,' and while there, examined a very large number of samples of air, water, etc., finding—as one would expect—that both air and water were singularly pure bacteriologically. He also made very complete bacteriological exam-

* The "experimental" guinea pigs only gained about 12 % when they might have been expected to gain about 21 %.

† *Ann. Inst. Pasteur*, xiii. (1899), 558.

inations of the intestines of a number of animals, including white bears, seals, sharks, eider-ducks, penguins, frigate birds, black gulls, sea-urchins, shrimps and others, with the following result:—The lower sea animals showed isolated *bacteria*; one bear and two seals gave *B. coli communis* but the great majority of the animals and all the birds except two were entirely free from micro-organisms; so that the question has at length been decisively answered. Pasteur was wrong, and it is possible for an animal to be perfectly healthy without the aid of bacteria.

On Wireless Telegraphy and Electric Discharge.

By Professor Henry Stroud, M A., D.Sc.

[Abstract of a paper read February 9th, 1900.]

This paper was prepared, by request, to give some account of the subject of wireless telegraphy. Since the author's public lecture (Oct. 5th, 1897) on the subject, considerable progress has been made in the application of what was then suitably termed the Lodge-Marconi system.

This system has been styled by Dr. Fleming the open-circuit method to distinguish it from the closed-circuit method devised by Preece many years ago and recently elaborated by Lodge in his experiments in "Magnetic Space Telegraphy."

This paper only dealt with the open-circuit method which has been brought into public use by the interesting large scale experiments of Signor Marconi.

The existence of electrical vibrations when a Leyden jar is discharged was noticed by Henry in 1842. Lord Kelvin gave the theory in 1853 stating the conditions for oscillation, and from 1857-1862 Feddersen published accounts demonstrating the oscillatory nature of the Leyden jar discharge, viz., by the use of a rapidly rotating mirror. The most direct way of showing the oscillatory character of the discharge is afforded, it is believed, by the figures which the author has obtained by the Lichtenberg method in continuation of Lord Armstrong's experiments. Some of these figures were shown on the table and in the lantern.

Hertz used in his experiments, the earlier of which were described about a dozen years ago, vibrations of the type of

those which occur when the inner and outer coatings of a charged Leyden jar are put into electrical connection. The electrical oscillations, or surgings to and fro, must, according to Maxwell, set up ether waves, which Hertz by his experiments demonstrated.

A brief description was then given of the Hertzian experiments, attention being directed to the oscillator or radiator and the resonator used as a receiver.

The coherer (electric eye), as devised by Branly in 1890, was then described and shown as a receiver. It depends for its action on the sensitiveness to electric waves of a bad electrical contact between oxidisable metals. The electrical resistance of the contact is much diminished when it is exposed to electric waves. A coherer consisting of a needle point and copper disc was next used, and lastly a relay coherer provided with horizontal collecting wires. With this last arrangement an experiment was made with the exciter on the table and the relay coherer at the far end of the theatre. When the wires of the exciter and coherer were parallel, or nearly so, the coherer readily responded, but by rotating either the exciter or the coherer, it was shown that the coherer ceased to respond when the wires were by no means parallel.

The history of Marconi's successful experiments was then briefly given. It was stated that his success was greatly due to two points: (1) the use of a high vertical wire attached to one side of the exciter and coherer, the other side of each being joined to earth; (2) the introduction of inductive coils between the coherer, relay and battery thus concentrating the wave energy taken up by the receiving wire upon the coherer.

Reference was made to the recent successful experiments across the Channel and during the Naval Manœuvres (1899). It was pointed out that for wireless telegraphy over considerable distances the idea of tuning the exciter to give

and the coherer to detect one particular period of vibration has not been applied up to the present. Should this be accomplished, the secret transmission of signals between two places will then be possible. It was, however, stated that both Marconi and Lodge have patented syntonic arrangements for use over short distances in connection with the open circuit method.

In conclusion experiments were shown with the Armstrong Wimshurst machine. Oscillatory discharges were transmitted along two wires, each about 60 feet in length, the ends of which were joined to various arrangements of different capacities. The luminosity of the wires was clearly seen in the darkened room. For these experiments the Wimshurst was in an upper room and the wires were brought down through a trap-door in the ceiling of the theatre.

On the Boulder Clay, Raised Beaches, and Associated Phenomena in the East of Durham.

By David Woolacott, M.Sc.

[Read March 1st, 1900.]

My purpose in this paper is to trace as far as possible the glacial and recent geological history of the East of Durham from the standpoint of the most recent geological theory.

That the surface of the Carboniferous and Permian strata of the counties of Durham and Northumberland had been carved into valleys before the glacial period began is evident from the most superficial study of the recent geology. The old rock surface lies now in many parts considerably below the present sea level, and is to a great extent covered up by several feet of clay, sand and the like. The following table showing the present sea-level and the depth of the old rock surface, as obtained in some of the borings in the district, will elucidate this more clearly (1).

Boring or Section.	Height above sea in feet.	Depth of superficial deposits in feet.
Boldon (A)	100	95
,, (B)	100	79
Brown's Buildings (Wash) ...	110	175
Chirton (Northumberland) (A)	102	181
,, ,, (B)	55	102
Fatfield House	100	72
Fulwell Waterworks	60	66 ?
Harraton	150	124
Harton (A)	79	118
,, (B)	85	116
,, (C)	75	75

Boring or Section.	Height above sea in feet.	Depth of superficial deposits in feet.
Hebburn	80	88
Hendon Banks...	80	80
Kibblesworth (Wash) ...	72	180
Lumley	165	79
Newcastle (under High Level)	0	52
Norwood New Pit (Wash) ...	16	156
Pelaw (Wash)	100	158
Percy Main	100	183
South Biddick	134	90
Springwell	455	9
Wallsend (A)	172	170
,, (B)	141	52
Washington	230	72

The course and apparent connections of these old valleys is peculiar, and throughout this paper they will be given the following names:—

(1) The so-called 'Wash,' in the Team Valley.

(2) The 'Tyne Valley,' in the course of the Tyne.

(3) The 'Jarrow Valley,' running from Washington to Jarrow.

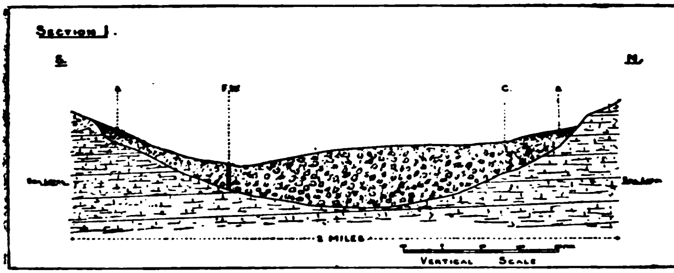
(4) The valley between Cleadon and Fulwell Hills—the 'Cleadon Valley.'

and lastly, the one running through Sunderland to Hendon—the 'Hendon Valley.'

It is perhaps worthy of note that no similar valley of any importance occurs between Hendon and Castle Eden.

The following sections across three of them show their nature better than any verbal description could possibly do. I would direct particular attention to the similarity between the sections across the Wash and the other valleys.

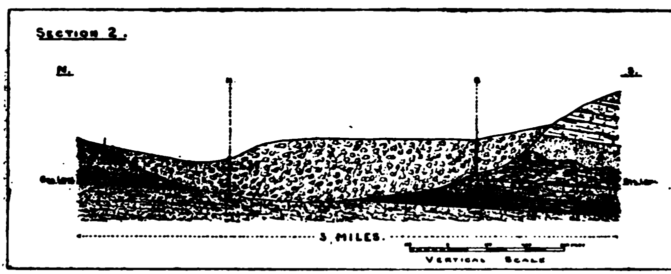
On a clear day most of them can be distinctly traced from the top of Cleadon or Fulwell Hills; when standing on one of these Magnesian Limestone Hills and looking towards the west, the Coal-measure fells stand out prominently in the distance, while between is a long stretch of level country. If we imagine the whole of the superficial deposits to be stripped off the ancient Tyne, Jarrow and Cleadon valleys would lie beneath us.



Section 1.—*From Fulwell to Cleadon Hills.*

a, a—Raised Beaches resting against old sea cliff.
F.W.—Fulwell Waterworks.
C.—Cleadon Village.

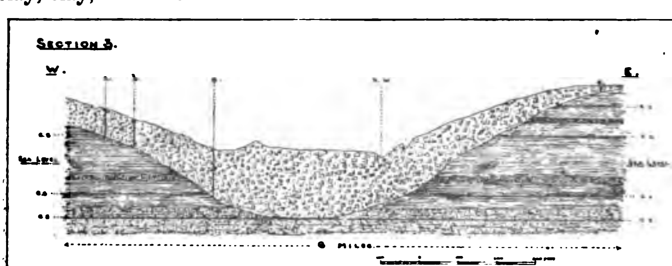
Valley in Permian strata filled in with stony boulder clay above which is a brick clay without boulders.



Section 2.—*From Corny Hill, South Shields, to W. Boldon.*

H—Harton Colliery. B—Boldon Colliery.

Valley in Coal-measures and Permian Strata filled up with boulder clay, clay, and sand.

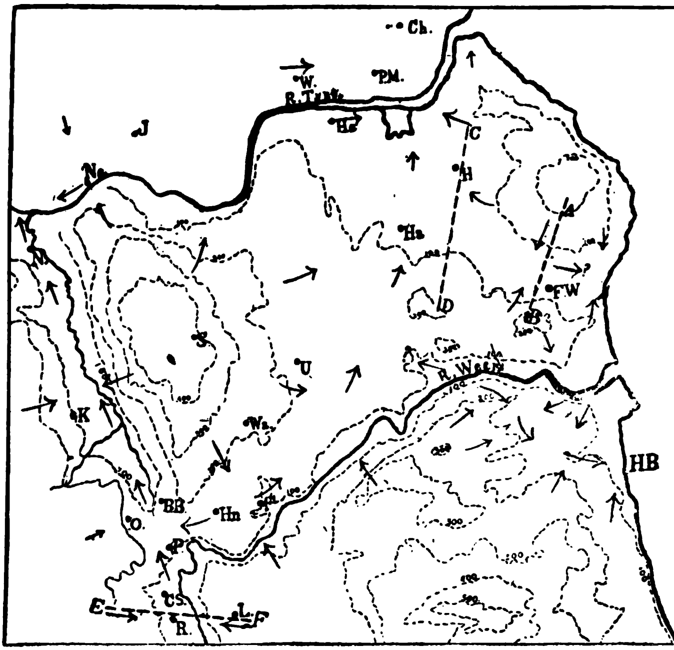


Section 3.—*Across Wash, from Paper by Boyd and Wood.*

a—Red Rose Hall Pit.
b—Borehole.
c—Borehole at Chester-le-Street.
d—Lumley Colliery.
R. W.—River Wear.

Valley in Coal-measures filled up with sand, gravel, and clay.

The direction of the sections is shown on the accompanying map of the district.



MAP OF 'WASH,' TYNE, CLEADON AND HENDON VALLEYS FILLED WITH SUPERFICIAL DEPOSITS.

Arrows indicate the probable present slope of pre-glacial rock-surface.

Borings, etc.—

BB, Brown's Buildings.
CS, Chester-le-Street.
Ch, Chirton.
F, Falfield House.
FW, Fulwell Waterworks.
H, Harton.
Ha, Boldon Colliery.
He, Hebburn.
Hn, Harraton.
HB, Hendon Banks.

J, Jesmond.
K, Kibblesworth.
L, Lumley.,
N, Norwood.
Ne, Newcastle,
O, Ouston.
PM, Percy Main.
R, Red Rose Hall.
S, Springwell.
W, Wallsend.
Wa, Washington.

A, *B*.—Section 1. Cleadon to Fulwell Hills.
C, *D*.—Section 2. From Corny Hill, S. Shields, to West Boldon.
E, *F*.—Section 3. Across Wash.

These old depressions prove to us that the district we are considering stood prior to the glacial period at a higher level than at present. Professor James Geikie in his 'Great Ice Age' shows that Scotland was then from 300 to 600 feet higher above the sea than it is now, and most geologists would admit that the British Isles were considerably higher at that time. If we admit this, then these old valleys may possibly have at some time formed part of the higher regions of a river system. It is otherwise very difficult to account for such a number of small branching valleys.

The 'Tyne Valley' was in all probability the principal one, while the others may have been all tributary to it. I would here draw attention to the importance of disconnecting the present names of the Wear and Tyne valleys from these ancient valleys, as both of these rivers have had to cut their present courses since glacial times. This is especially noticeable in the case of the Wear, its present course being almost entirely post-glacial. At some future time it will be interesting to trace the old valleys in the district to the north and west, and see their course and connections, if any, with the ones at present being considered. This paper cannot therefore be considered as final, as subsequent research may alter to some extent the views expressed herein, with regard to the direction, and slope of these preglacial depressions.

During the glacial period the surface features of these valleys were in all probability slightly altered, but that no changes of any great moment took place, we may be tolerably certain. That some action did take place we have evidence in the 'Wash,' as in the paper on it by Messrs. Wood and Boyd (2) the surface of a sandstone forming the rocky bottom of this valley is referred to as being 'furrowed and polished in rough and scored outlines.'

It seems to be moreover tolerably certain that these preglacial valleys were produced in the same way as such features are formed at the present time, namely, by the action of

running water. They were in all probability the old drainage valleys of the district, and we may accept without much hesitation that they were not formed by any great and sudden current of water nor scooped out by the action of glaciers.

The low-lying ground between the Wash and Jarrow valleys, the rocky surface of which slopes into both of them, may be accounted for in two ways. They may have been two separate valleys with a watershed between. It seems probable that the Wear Valley glacier came across the lowest ground between the two valleys, and in doing so it would, to a very slight extent, lower the barrier between them. This was possibly due to a larger mass of ice moving southwards, or down the Tyne Valley, damming back the one moving north along the course of the Wash. The Northern glacier seems undoubtedly to have come down between Cleadon and Fulwell Hills and through Sunderland to Hendon. This, however, admits of a very different explanation. It is possible that these two valleys were at one time connected, and that the connection between them has been removed by earth movements; and this seems the more probable when we take into account that the preglacial Tyne valley near Newcastle also appears to have a double slope.

That earth movements have taken place since glacial times we have certain evidence in the raised beaches on Cleadon and Fulwell Hills, to be hereafter more fully referred to, and that these movements may have been differential between the east and west is possible; indeed considerable support for this view is obtained from the fact that the borings prove that the rock surface lies many feet deeper in the Team Valley than in those nearer the sea, indeed the thickest deposits beneath sea-level in the district now being considered is found in the Team Valley. In the following Table is shown the greater depths of the rock-surface beneath sea-level in the district:—

Boring.	Depth of superficial deposits beneath sea level in feet.
Norwood New Pit (Team Valley)	140
Kibblesworth ,, ,, ...	108
Percy Main (Northumberland)	83
Chirton ,, ...	79
Newcastle (High Level)	52
Harton	39

These borings appear to prove that differential earth-movements between the east and west of Durham have occurred since the preglacial valleys were formed. Of course, the movements that produced the raised beaches and this phenomena may not be connected—although it would seem most probable that they were—in any way with one another, nor with the production of the barrier before referred to.

These ancient waterways are now filled up with deposits of a very varying character. The nature of these is well shown by the following typical borings in the district (1).

HARTON.	Ft.	Ins.
Soil	0	6
Yellow clay ...	2	4
Leafy clay ...	9	4
Stony ,, ...	67	10
	———	——— *
	1	4
Leafy ,, ...	16	0
Loamy ,, ...	2	6
Sand	1	0
Loamy sand ...	12	4
Leafy clay ...	1	4
Loamy sand ...	0	8
Marl and Limestone	4	0
	119	2

PERCY MAIN.	Ft.	Ins.
Blue stony clay ...	78	6
Sand	2	1
Blue stony clay ...	19	5
	———	——— *
	48	8
Dry stony clay ...	5	6
Leafy clay ...	2	6
Sand	1	8
Sand and clay ...	2	3
Stony clay ...	2	4
Gravel	1	0
Sand and water ...	1	11
Leafy clay ...	9	6
Sand	4	1
Gravel	3	10
	183	3

* Sea level.

These deposits may be roughly divided into two main parts:—

(1) Stony Boulder Clay.

(2) Deposits of Sand and Gravel, and washed-up Boulder clay.

The first is an unstratified, firm, brown clay, full of stones, varying in size from a few grains to several tons. Probably the best section of it is exposed at Hendon Banks to the south of Sunderland. The stones in it prove that, at this point at any rate, the agent producing it came from the direction of the Cheviots (3). This clay often rests on the surface of the rock and covers the Magnesian Limestone hills of the district. At Hendon there is at present exposed an interesting section of its lowest part. It shows that resting on the hard surface of the limestone is a marl of about 1 foot in thickness, containing numerous angular fragments of limestone, and passing gradually up into the clay. This appears to show that a scraping and grinding action took place beneath the boulder clay when it was being formed. The 'marl and limestone' in the Harton boring is probably of the same nature as that at Hendon.

It is generally admitted that clay of this character—the only kind that should be called 'boulder clay'—was produced during a time when England was to a great extent covered with ice. A geologist of Sunderland (Mr. G. W. Card, A.R.S.M.), but now of Australia, informs me that he washed a pound or two of the clay at Hendon for micro-organisms with absolutely negative results. Only on one occasion is there any record of any animal remains being found in it. These were a few small fragments of Cyprina Islandica found in it near South Shields by Mr. Howse, but it is hardly possible to draw any definite conclusions from them (4). It is also interesting to note that in a section recently exposed in Castle Eden Dene by the excavations for the new railway from Hartlepool to Seaham, and examined

by Mr. D. M. Chapman, B.Sc. and myself, the line of demarcation between the northern brown boulder clay and the red of the Tees Valley appeared to be in that Dene.

The second type of deposits may be subdivided into two classes: (1) materials deposited by water before, during and immediately after the glacial period, (2) materials mainly consisting of washings from the boulder clay and produced since glacial times.

The first kind of deposits would occur in three distinct positions—below, in, or upon the surface of the boulder clay. Those below, as in the 'Wash,' can probably be accounted for in the following way. Before the ice overspread the lower parts of Northumberland and Durham it appears certain that there would be many streams of water flowing along the old valleys and depositing in them stones, sand and sandy clay.

Also during the period observations, which have been made in districts where glacial conditions prevail now, lead us to suppose that streams of water varying in amount with climate and season would flow along the bottom of the glaciers and thus form the beds of sand, gravel and sandy clay that are found intercalated with the clay.

As far as the district under consideration goes there appears to be no evidence of any interglacial period or periods, of course some of the thicker deposits of sand and gravel in the clay may and probably do represent times when the ice was melting more quickly than others, but whether they can in any sense be considered as interglacial is very doubtful. Professor Geikie in his 'Great Ice Age' endeavours to prove that there were at least three interglacial periods, the middle one of which left marked effects in Yorkshire and other parts of England, but of such we have in our district no certain evidence.

The extensive spread of sand and gravel—drift—that occurs resting on the boulder clay in the south of Durham was most probably produced during the time when the enor-

mous accumulations of ice and snow were melting at the end of the Ice Age.

We now pass on to consider the deposits formed since the end of the glacial period. In the formation of these the sea has played a considerable part. Some geologists accept that during the glacial period nearly the whole of the British Isles were submerged beneath the sea; Howse in a paper on the 'Glaciation of the Counties of Northumberland and Durham' (4) suggests the submergence of the whole of the North of England excepting the Cheviots and Pennines, but Professor Lebour states in his 'Geology of Northumberland and Durham' (5) that there is nothing to prove that the upper drift gravels and sands of these counties were produced by marine agency—if they have been a submergence of some 500 feet would be necessary to account for them.

We have, however, certain evidence of one change of level since the period of glaciation ended. It is that indicated by the raised beaches of Cleadon and Fulwell Hills (see Section 1). An old sea beach can be traced almost completely round Cleadon Hills at levels between 100 and 150 feet, and an old sea-cliff occurs at the latter height (6).

On Fulwell Hill a much better exposure of the same feature occurs (7). In the quarries on this hill a deposit of sand and gravel with washings of boulder clay occurs running up to an old sea-cliff at a level of about 150 feet. The stones occurring in it are those common to the boulder clay, only a greater proportion of Magnesian Limestone ones are present. Several fragments of Cyprina Islandica and many rolled flints have been found in this deposit. The occurrence of the latter is rather a problem in local geology. They may be as suggested by Mr. Howse washed up from a deposit on Trow Rocks, which that geologist considers to be ice-borne drift from Scandinavia. The nearest places where flints cccur are Flamborough Head, North of Ireland, Denmark and Aberdeen. It is difficult to see how they can have come

from any of these places. When flints are found in any district, they must have been, as far as we know, carried to that place by some agency, as ice or sea-weed, or they might prove the former extension of the chalk over that area, or lastly be brought by man. The first method seems to be the most acceptable of these in the present case.

During the time that the sea was washing against the old cliffs all the land in the East of Durham must have been submerged beneath the sea, and much of the boulder clay that covered the slopes of the hills and valleys would be washed up, the materials composing it being reassorted and redeposited. In this way much of the true boulder clay must have been denuded. To this period is, therefore, probably to be referred the clay that occurs above the true boulder clay in the valleys of Jarrow and Cleadon. It contains few stones, has a tendency to prismatic jointing, and is of a bluish colour. It is the brick clay of the district, and was very probably formed during the period of submergences. Also to the same period are to be referred the beds of sand and gravel and sandy clay so frequently met with in Sunderland, (δ) resting on the boulder clay, and those that occur along the coast from Hendon to Castle Eden.

Since glacial times, as we have before indicated, the present course of the River Wear, in its lower reaches at least, has in all probability been cut. It has at Finchale and elsewhere denuded the Coal-measures; while leaving the valley of the Wash, down which the pre-glacial drainage of that area flowed, it has cut its way across into the Jarrow valley, and then, after flowing across it, has carved a way for itself through the boulder clay and Magnesian Limestone to the sea at Sunderland; and the Tyne, flowing much above the level of its old course, has had to cut for itself a course since the ice retreated from the lower levels to the Cheviots and Pennines, and then gradually faded away. The Tyne, however, probably flows much more in the direction of its old course than the Wear.

Finally, taking the ancient and the present valleys of the district into consideration, together with the deposits that occur in them, and on the slopes of the valleys surrounding them, there appears to be little that cannot be given a fairly satisfactory explanation from the standpoint of present-day geological theory, while a few phenomena apparently lend considerable support thereto. Theory, however, must always be subservient to fact, and it is quite possible that a different explanation might be given to some of the phenomena discussed in this paper.

REFERENCES.

[1] *Borings and Sinkings in the Counties of Northumberland and Durham.*

[2] *Transactions* of the North of England Inst. of Mining and Mechanical Engineers, vol. xiii.

[3] *Geology of North-East Durham.* By D. Woolacott, page 68.

[4] "On the Glaciation of Northumberland and Durham." By R. Howse. *Transactions* of North of England Inst. of Mining and Mechanical Engineers, vol. xiii.

[5] *Geology of Northumberland and Durham*, page 24.

[6] *Ibid.*, page 19.

[7] "On a portion of a Raised Beach on the Fulwell Hills near Sunderland." By D. Woolacott. *Natural History Transactions of Northumberland, Durham, and Newcastle-on-Tyne*, vol. xiii. pt. 2.

[8] See "Geological Map of Sunderland." *Annual Report of Health and Sanitary Condition of Sunderland.*" By Dr. Scurfield.

On the Production of Oxalic Acid by Bacteria.

By Professor M. C. Potter, M.A.

[Read March 1st, 1900.]

Oxalic acid, a well known product of metabolism of the higher plants and some fungi, has hitherto not been observed to be produced by bacteria. An account was given of some experiments showing that oxalic acid was formed by at least one bacterium (*Pseudomonas destructans* Potter) when growing in sugar containing solutions such as the expressed cell-sap of turnips. Small pieces of turnip steamed until soft in the presence of an excess of calcium carbonate yielded a solution free from oxalic acid, when however, the solution was sterilised and inoculated with a pure culture of *P. destructans*, under sterile conditions, it was found, on addition of calcium chloride, to give a precipitate of calcium oxalate in the presence of acetic acid. The calcium precipitate when mixed with manganese dioxide and treated with sulphuric acid yielded carbonic acid, which furnished a further confirmatory test of the presence of oxalic acid.

Similar tests showed that *P. destructans* also sets up an oxalic fermentation in Pasteur's solution containing cane sugar.

In a former paper it was shown that *P. destructans* lives as a parasite on the turnip roots producing a cytase which destroys the cell-wall. It was now proved that this schizomycete also sets up an oxalic fermentation, and that the oxalic acid acts as a powerful toxin in killing the protoplasm of the host-plant. Three series of test-tubes with about 10 c.c.

of a Pasteur's solution, in which a pure culture of *P. destructans* had been introduced, were prepared. To the first series sufficient calcium carbonate was added to neutralise the oxalic acid. The first and second series were then sterilised by discontinuous boiling, and when cool sterile sections of turnip were introduced. In the first series the cells remained quite normal after twenty-four hours. In the second series the protoplasm was dead, faintly brown, and had contracted from the cell-wall. In the third series of test-tubes the oxalic acid was not neutralised nor the cytase destroyed by boiling, and in this case the cells exhibited, after the same interval of time, very plainly the combined action of the cytase and toxin. The cell-walls showed complete dissociation, and the protoplasm was strongly contracted.

It was further argued that the calcium pectate of the middle lamella would neutralise the oxalic acid produced by the bacteria, and thus enable them to continue their existence and so pass on from cell to cell.

The production of a cytase and toxin by bacteria and their entrance by this means into living plant tissues establishes the existence of bacterial diseases of plants precisely homologous, in those respects, to those of some of the higher fungi.

Robert Harley.

By Walter Frewen Lord, B.A.

[Abstract of a paper read May 17th, 1900.]

The examination of the career of Robert Harley in the light of the most recent comparison of available authorities on the period of Queen Anne has materially altered the perspective in which we have been accustomed to view the leading characters of the reign. It has become abundantly clear that the *coup d'etat* of 1710, hitherto supposed to have been the work of Harley was really a long-prepared measure of the Queen. Its object (kept steadily in view, since the enforced dismissal of Harley in 1708, and perhaps from a date even earlier) was to rid the Queen of the tyranny of the Whig junto. Harley succeeded to high office in the natural course of events, but exercised practically no control over political affairs owing to his indolence, his intemperance, and his genuine unfitness for dealing with matters of high policy. Nevertheless, between the years 1710 and 1714, the Treaty of Utrecht was negotiated and concluded. This Treaty, so often blamed as an abandonment of the favourable position secured for England by the successful conduct of the war, is rather to be regarded as an extraordinary performance of St. John's. Inasmuch as Harley was incapable of conducting business, the duty fell to St. John. For the same reason, open courses were abandoned, and the Treaty was conducted to an issue by means of secret agents. The employment of such agencies might be blameworthy in ordinary circumstances, but could hardly be said to be so in the years 1710-1714. During these years there was practically no other means of getting work done; and the statement made by St. John and accepted by Rémusat that from the autumn of 1713 there was practically no such thing in existence as the British government has been found to be substantially correct.

PROCEEDINGS

OF THE

University of Durham Philosophical Society

(ABSTRACTED FROM THE MINUTES).

November 2nd, 1899.

(AT THE COLLEGE OF SCIENCE, PRINCIPAL GURNEY IN THE CHAIR.)

The Treasurer read his report, which showed that the financial position of the Society was satisfactory.

The following officers were elected for the session 1899-1900 :—

President :

THE VERY REV. THE WARDEN.

Vice-Presidents :

PROFESSOR P. PHILLIPS BEDSON, M.A., D.Sc.
PROFESSOR H. P. GURNEY, M.A., D.C.L.
F. B. JEVONS, M.A., D. LITT.
A. L. PERCIVAL, M.A., M.D.
PROFESSOR G. H. PHILIPSON, M.A., M.D., D.C.L.
REV. H. B. TRISTRAM, M.A., D.D., F.R.S.

Committee :

J. T. MERTZ, PH.D., D.C.L.
PROFESSOR G. S. BRADY, M.D., D.Sc., F.R.S.
PROFESSOR R. A. SAMPSON, M.A.
ROBERT A. BOLAM, M.D.
PROFESSOR R. HOWDEN, M.D.
MISS SMITH, B.Sc.

Chairman (*Section A*) ... PROFESSOR H. STROUD, D.Sc.
(*Section B*) ... PROFESSOR POTTER.

Hon. Secretaries :

F. C. GARRETT, M.Sc. (Coll. Sc.)
G. MACK (Coll. Med.)

Sec., Section A : T BAKER, B.Sc. *Sec., Section B :* G. POTTS, B.Sc.

The following candidates were elected members of the Society :—

H. E. Watt. T. E. Hodgkin. G. Potts, B Sc.

Professor Louis read a paper on 'The Blow Gun as used by the Aboriginal Tribes in the Malay Peninsula.'

Professor Potter gave an account of the curious old Javanese fable of the Upas tree, and described its true poisonous qualities as shown by actual experiment.

November 30th, 1899.

(At the College of Science, Professor Potter in the Chair.)

The following candidate was elected a member of the Society :—

Professor Middleton.

Mr. L. E. Hodgkin read a paper on 'Some of the Theories of Preformation and Epigensis.'

December 15th, 1899.

Chemical and Physical Section.

(At the College of Science, Professor Stroud in the Chair.)

The following candidates were elected members of the Society :—

Messrs. Woolf and Robson.

Dr. Percival read a paper on 'Certain colour phenomena caused by intermittent stimulation with white light.'

Mr. S. H. Collins exhibited a series of photographs of Indian Cattle.

January 18th, 1900.

(At the College of Science, Professor Louis in the Chair.)

Dr. Lyle was elected a member of the Society.

Professor Lebour read a paper on 'The Pleasures of Boring,' illustrating what he had to say by the aid of lantern and slides.

January 25th, 1900.

(At the College of Medicine, Professor Potter in the Chair.)

Biological Section.

Professor Howden exhibited a series of micro-photographs illustrative of the Maturation, Fertilization, and Segmentation in Echinus Esculentus.

Mr. Garrett read a paper entitled 'Animal Life without Bacteria.'

Dr. Bolam exhibited and explained the mechanism and use of sundry physiological instruments of recent invention.

February 9th, 1900.

Chemical and Physical Section.

(At the College of Science, Professor Stroud in the Chair.)

Mr. W. Frewen Lord was elected a member of the Society.

Professor Stroud read a paper on 'Wireless Telegraphy and the Electric discharge,' illustrating his remarks by the aid of lantern slides, and some very interesting experiments with the Armstrong-Wimshurst machine.

March 1st, 1900.

Biological Section.

(At the College of Science, Professor Potter in the Chair.)

Mr. R. B. Greig was elected a member of the Society.

Mr. Woolacott read a paper on 'The Boulder Clay, Raised Beaches, and Associated Phenomena in the East of Durham.' He illustrated his paper with diagrams, lantern slides and specimens.

Professor Potter exhibited various Intumescences on the roots of turnips, and then read a paper on 'An undescribed Phoma disease of the Swede.'

The author described a fungoid attack of the Swede crop, noted especially at the Northumberland County Council's Experimental Farm at Cockle Park and in other districts in Northumberland. The chief characteristics of the attack were the large fissures in the root surrounded by a discoloured edge on which numerous pycnidia were to be observed. The spores were successfully cultivated, and the disease induced with the subsequent production of pycnidia and spores in sound roots. A full account has been published in the Journal of the Board of Agriculture, March, 1899.

March 16th, 1900.

CHEMICAL AND PHYSICAL SECTION.

(AT THE COLLEGE OF SCIENCE, PROFESSOR STROUD IN THE CHAIR.)

Mr. Shaw read a paper on 'The Structure of Metals' and exhibited many magnificent micro-photographs bearing upon the subject.

May 17th 1900.

(AT UNIVERSITY COLLEGE, DR. PLUMMER IN THE CHAIR.)

Mr. Walter Frewen Lord read a paper on 'Robert Harley.'

LIST OF MEMBERS OF THE SOCIETY.

* *Denotes an original member.*

*ARMOUR, A. L.
ARNISON, W. D., M.D.
*ASHTON, A. W., B.Sc.
*BAKER, T., B.Sc.
BEATTIE, THOMAS, M D.
*BEDSON, PROFESSOR P. P., M.A., D.Sc. (*Vice-President*).
BETTS, R. F., B.Sc.
*BOLAM, R. A., M.D.
*BRADY, PROFESSOR G. S., M.D., LL.D., D.Sc., F.R.S.
*BROWN, G. M., B.A., B.Sc.
*BULLERWELL, J. W., M.Sc.
CADMAN, J., B.Sc.
*CAIRNS, MRS. C. W., B.Sc.
CAIRNS, C. W., M.Sc.
*CAMPBELL, WILLIAM, B.Sc.
CARRICK, D. R.
*CAUNT, G. W., B.A.
*CHAPLIN, G. P., B.Sc.
COLLINS, S. H.
*COOPER, J., B.Sc.
*DALE, SIR DAVID, BART, D.C.L., D.L.
DIXON, J. G.
DODDS, R., B.Sc.
DUNN, J. T., D.Sc.
ELLERSHAW, REV. H., M.A.
ELSDON, E. J.
FLETCHER, REV. MARK, M.A.
*FOWLER, REV. J. T., M.A., D.C.L.
*GARRETT, F. C., M.Sc, (*Secretary.*)
*GRAVELL, JOHN.
*GRAY, W. R. H., M.A.
GREIG, R. B.
*GURNEY, REV. PRINCIPAL H. P. M.A., D.C.L. (*Vice-President*).
HALL, G., B.Sc.
*HALLAWAY, R. R., B.Sc.
HARDIE, T.
HATTON, R. G.
*HAVELOCK, T. H., B.A., B.Sc.
HAYWARD, J. W., M.Sc.
*HEAWOOD, P. J., M.A.
HILTON, W. K., M.A.
HODGKIN, T. E.
*HOWDEN, PROFESSOR R., M.A., M.B.
HOWSON, C.
HUNTER, T.
JACOBS, J.
*JESSOP, PROFESSOR C. M., M.A.
*JEVONS, F. B., M.A., D.LITT. (*Vice-President*).
*KITCHIN, THE VERY REV. DEAN, M.A., D.D., (*President*).
*KNIGHT, R.
LAWS, A. R.
*LEBOUR, PROFESSOR, G. A., M.A., M.Sc.
*LINTON, A., B.Sc.
LORD, W. FREWEN, B.A.
*LOUIS, PROFESSOR HENRY, M.A., A.R.S.M.
LOVIBOND, J. L., B.A.

Lyle, R. P. Ranken, M.D.
Mack, G. (*Secretary*).
McGowan, E.
Maxwell, W. W.
*Meek, A., M.Sc.
*Mellanby, A. L., M.Sc.
Merivale, J. H., M.A.
Middleton, Professor T. H.
*Merz, J. T., Ph.D., D.C.L.
Mundella, V. A., M.A., B.Sc.
*Murray, Professor George, M.A., M.D.
Oliver, Professor Thomas, M.A., M.D.
*Patterson, R. J., M.Sc.
Pearce, Rev. R. J., M.A., D.D.
*Peacock, Miss M. I.
*Percival, A. L., M.D. (*Vice-President*).
*Philipson, Professor Sir G. H., M.A., M.D., D.C.L. (*Vice-President*).
Philipson, W.
*Plummer, Rev. A., M.A., D.D.
*Potter, Professor M. C., M.A.
Potts, G., B.Sc.
Proctor, A. H.
*Redmayne, R. R., M.A.
*Richards, W., A.Sc.
Robson, R.
Rollin, C., B.Sc.
*Sampson, Professor R. A., M.A.
*Shaw, Saville, M. Sc.
Smith, Miss K. A., B.Sc.
*Stockdale, H. F.
*Stroud, Professor Henry, M.A. D.Sc.
Thomson, W., M.A.
Thornton, W. M., M.Sc.
*Todd, J. J.
Treble, R. L., B.Sc.
*Tristram, Rev. Canon, D.D. F.R.S. (*Vice-President*).
Turner, G. G., M.B.
*Turnbull, W., M.B.
*Urwick, W. E., M.A.
*Wade, Thomas.
Wakerley, F.
Watt, H. E., B.Sc.
Watson, J., B.Sc.
*Watson, F. B., M.A., M.Sc.
*Weighton, Professor R. L., M.A.
Welford, R., M.A.
Wilcox, F. A., B.Sc.
Woolacott, David, M.Sc.
Woolf, J.
Wright, Professor Mark R., M.A.

UNIVERSITY OF DURHAM PHILOSOPHICAL SOCIETY.

Income and Expenditure Account. Session 1899-1900.

Income.	£	s.	d.	Expenditure.	£	s.	d.
To Balance from Session 1898-1899 ...	17	11	11	By Printing and Issuing Notices of Meetings and Circulars	4	14	10
,, 1 Subscription for Session 1897-1898 ...	0	5	0	,, Printing *Transactions*	7	4	0
,, 3 ,, ,, ,, 1898-1899... ...	0	15	0	,, Expenses of holding Meetings	3	5	0
,, 89 ,, ,, ,, 1899-1900 ...	22	5	0	,, Secretarial Expenses	1	3	2
,, 2 ,, ,, ,, 1900-1901	0	10	0	,, Assistant Treasurer's Commission	0	12	0
,, Sale of *Transactions*	0	1	0	,, Balance in Treasurer's Hands	24	8	11
	£41	7	11		£41	7	11

Examined and found correct.

November 1*st*, 1900.

HENRY LOUIS, *Auditor*.

RULES OF THE SOCIETY.

(Revised to December 1st, 1900.)

Name.

I.—The Society shall be called the University of Durham Philosophical Society.

Objects.

II.—The objects of the Society shall be (*a*) the promotion of research; (*b*) the communication and discussion of facts and ideas bearing upon scientific and philosophic questions; (*c*) the exhibition of specimens, apparatus, and books; and (*d*) friendly intercourse between workers in different fields.

Management.

III.—The business of the Society shall be managed by a Committee elected at the meeting in the month of November, consisting of the officers, sectional officers, and six other Members of whom three at least shall be re-elected each year.

Officers.

IV.—The officers of the Society shall be a President, six Vice-Presidents, an Editor, and two Secretaries (one of whom shall also act as Treasurer). The President and Vice-Presidents shall be elected annually, but not more than four of the Vice-Presidents shall be re-elected in each year; the Secretaries shall be appointed for two years, but one of them shall retire in each year. All elections shall be by ballot. All officers shall be eligible for re-election.

Membership.

V.—All past and present students in the Colleges of the University, all past and present members of the University, and of the College Staffs and Councils shall be eligible for membership of the Society. Candidates for election must be nominated in writing by two Members of the Society, and their nominations must be in the Secretaries' hands at least three days before the meeting of the Society at which they are to be proposed. Elections shall be by ballot, and a candidate shall not be elected unless at least three-fourths of the votes given are in his or her favour.

Meetings.

VI.—The Committee shall decide the place and time of each meeting, but in the course of the year one meeting at least shall be held in Newcastle, and one at least in Durham. The usual hour of the meeting shall be 7 p.m., and the usual day the First Thursday in November, December, February, March, May, and June. It shall be the duty of the Socretaries to send each Member a written notice three days before each meeting.

Subscription.

VII.—Each Member shall pay an annual subscription of Five Shillings. Members whose subscription for the current year are unpaid shall not be entitled to receive copies of the Society's Publications, and those whose subscription are two years in arrears may be struck off the List of Members by the Treasurer.

Visitors.

VIII.—Members shall have the privilege of introducing friends.

INDEX.